

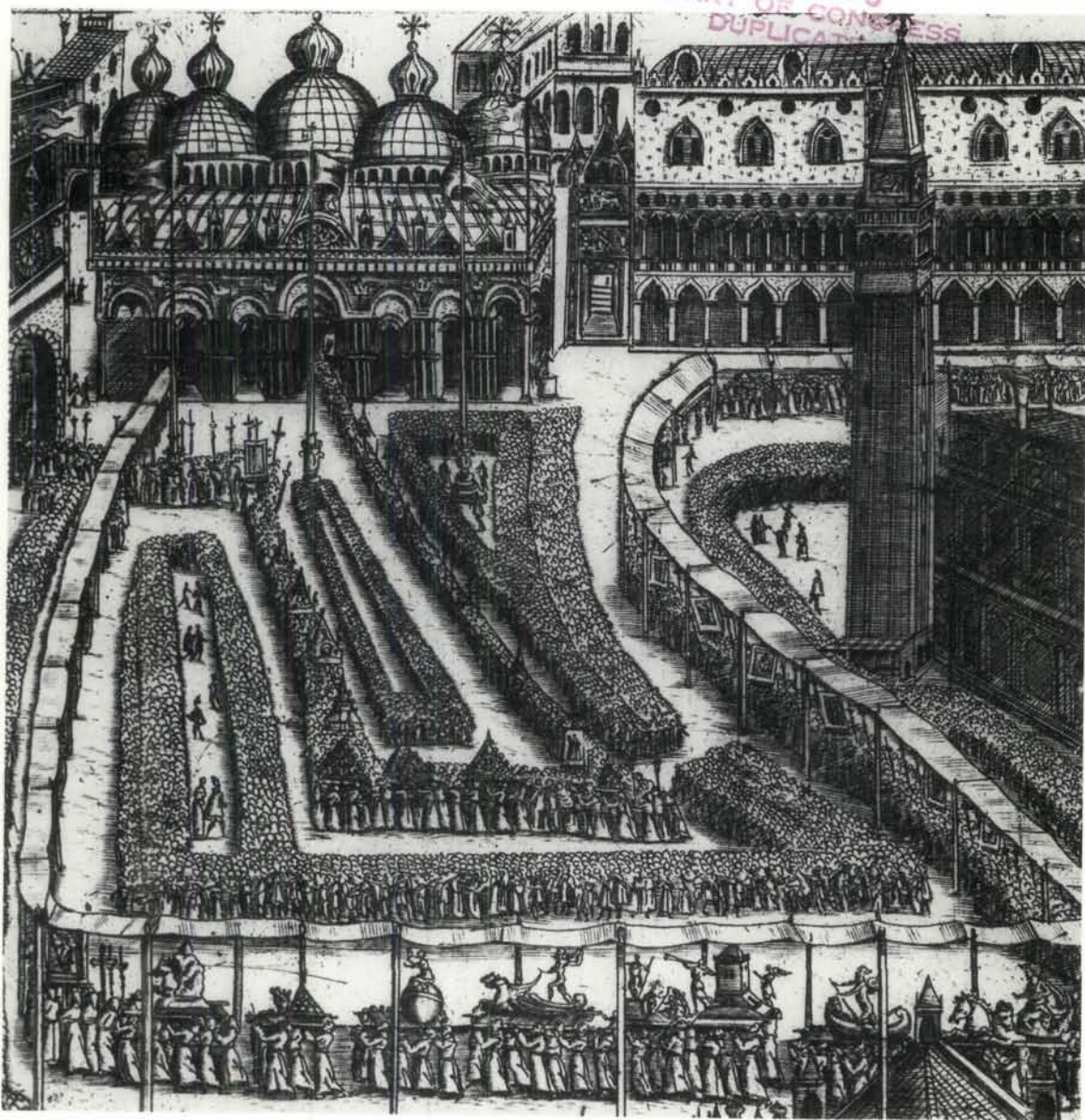
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The American Historical Review appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located in Ballantine Hall, Indiana University, Bloomington, Indiana 47401.

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes over \$30,000, \$40.00 annually; \$20,000-\$29,999, \$35.00; \$15,000-\$19,999, \$30.00; \$10,000-\$14,999, \$20.00; below \$10,000 and joint memberships, \$10.00; associate (non-historian) \$20.00; life \$650. The proportion of dues allocated to the *AHR* is \$15.00. Subscription rates effective for volume 82: Class I, *American Historical Review* only, United States, Canada, and Mexico \$39.00, foreign \$43.00. Further information on membership, subscriptions, and the ordering of back issues is contained on the two pages—1(a) and 2(a)—immediately preceding the advertisements.

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The Renaissance and the Drama of Western History

WILLIAM J. BOUWSMA

I SHOULD LIKE TO DISCUSS A REMARKABLE HISTORIOGRAPHICAL EVENT—an event so recent that it may have escaped general notice, yet of considerable importance both for historians and for the larger culture of which we are a part. This event is the collapse of the traditional dramatic organization of Western history. We have long depended upon it, as inhabitants of the modern world, to put the present into some distant temporal perspective and, as professional historians pursuing our particular investigations, to provide us with some sense of how the various fields of history are related to each other as parts of a larger whole. Thus, the subject seems appropriate for a general session of our annual meeting. The subject is also appropriate for me, as a historian of the Renaissance, because of the pivotal position of the Renaissance in the traditional pattern. Indeed, the historian of the Renaissance has long been the principal guardian of that pattern. But historians of the Renaissance have lately been unable—or unwilling—to fulfill this old responsibility. Hence, this essay is also a kind of oblique professional autobiography, though I point this out only for the sake of candor, not as a further inducement to your attention.

Nothing seemed less likely than this development when I entered the profession some thirty years ago or, indeed, before the last two decades. Earlier in this century, the Burckhardtian vision of the importance of the Renaissance for the formation of the modern world had been under attack in the “revolt of the medievalists”; and in 1940 Wallace K. Ferguson had described the Renaissance as “the most intractable problem child of historiography.”¹ But Ferguson had himself never been without hope for straightening out his problem child; and less than a decade later, after studying the history of the case from many directions, he predicted for it a tranquil and prosperous maturity. The time was ripe, he declared, for “a new and more comprehensive synthesis.”² The revolt of the medievalists had apparently

This presidential address was delivered at the annual meeting of the American Historical Association in San Francisco, December 28, 1978. I should like to acknowledge at the outset the helpful criticism this paper received from Thomas A. Brady, Jr. of the University of Oregon and from my Berkeley colleagues Gene Brucker and Randolph Starn.

¹ Ferguson, *The Renaissance* (New York, 1940), 2.

² Ferguson, *The Renaissance in Historical Thought* (Boston, 1948), 389.

been beaten back; indeed, by teaching us greater care in distinguishing the new from the old, they seemed only to have strengthened our sense of the originality and modernity of the Renaissance. In the years after the war a group of unusually distinguished scholars brought new excitement to Renaissance studies; the concreteness and depth of their learning seemed to confirm Ferguson's expectations.³

During the fifties, therefore, it was common for Renaissance specialists from various disciplines to celebrate, by reading papers to each other, their triumph over the medievalists and the world-historical significance of the Renaissance. Our agreement was remarkable. The editor of one volume of such papers noted with satisfaction "the virtual disappearance of the disposition to deny that there was a Renaissance." And he ventured to predict, obviously recalling controversies now happily over, "that future soldier scholars will beat their swords into ploughshares and that what has long been the Renaissance battleground will be transformed into a plain of peace and plenty." On the other hand, he also hinted that the occasion evoking these papers was a bit dull. "The atmosphere of charitable catholicity was so all pervading during the symposium," he remarked, "that even the moderators' valiant efforts to provoke controversy were largely futile."⁴ That the Renaissance was the critical episode in a dramatic process that would culminate in ourselves had become an orthodoxy that few cared—or dared—to question.

The notion of an abiding consensus among historians of any complex subject may now seem rather surprising, and this agreeable situation was probably in part a reflection of the general consensus of the Eisenhower years, when we were all beating our swords into ploughshares. That same irenic mood, that same amiable but slightly complacent consensus, also left its mark on other fields of history. The gentle complaint of our editor, disappointed in his hopes for a little fun at a scholarly symposium, hinted at the charge of dullness brought by bored professors against their boring students of the silent generation—upon which we would soon enough be looking back with a degree of nostalgia. For since the 1960s the world around us has dramatically changed, and with it historiography.

These two sets of changes are not unrelated, and the result for the Renaissance has been rather different from what Ferguson foresaw. In his vision the Renaissance was to retain its pivotal position in the old scenario, but our knowledge of it would be better pulled together. But this has not occurred. Although the consensus of the golden 1950s has not been seriously challenged, we are now remarkably indifferent to the world-historical importance of the

³ For some of the works that particularly influenced me at this time, in addition to those of Ferguson, see Paul Oskar Kristeller, *The Classics and Renaissance Thought* (Cambridge, Mass., 1955); Hans Baron, *The Crisis of the Early Italian Renaissance: Civic Humanism and Republican Liberty in an Age of Classicism and Tyranny*, 2 vols. (Princeton, 1955); Eugenio Garin, *L'umanesimo italiano* (Bari, 1958); and the various essays of Erwin Panofsky, especially "Renaissance and Renascences," *Kenyon Review*, 6 (1944): 201–36.

⁴ Tinsley Helton, ed. *The Renaissance: A Reconsideration of the Theories and Interpretations of the Age* (Madison, Wisc., 1961), xi–xii. The papers in this volume were presented at a symposium at the University of Wisconsin, Milwaukee in 1959. For other symposia, see *The Renaissance: A Symposium* (New York, 1953); and Bernard O'Kelly, ed., *The Renaissance Image of Man and the World* (Columbus, Ohio, 1966).

Renaissance.⁵ We go about our particular investigations as though the Renaissance problem had evaporated; we neither affirm nor bother to deny that there was a Renaissance. And the venerable Renaissance label has become little more than an administrative convenience, a kind of blanket under which we huddle together less out of mutual attraction than because, for certain purposes, we have nowhere else to go.⁶

I DO NOT MEAN TO EXAGGERATE the abruptness of this development. In retrospect we can see that the role of historians in the postwar rehabilitation of the Renaissance was always somewhat ambiguous. We accepted what was said in praise of the Renaissance by representatives of other humanistic disciplines; the importance of the Renaissance for them enhanced our own importance. But, like Garrett B. Mattingly on one such occasion, we were sometimes "puzzled" about what we might contribute to a Renaissance symposium.⁷ The normal skepticism of a professional historian in the presence of large views has now given way, however, to agnosticism and even indifference about what was once the central claim of Renaissance scholarship.

This result may have been implicit in Ferguson's call for synthesis, with which most of us were sympathetic even in the 1950s without fully realizing its implications. It implied the integration of all of our data, an aspiration that seemed unexceptionable. But the ideal of "synthesis"—at least for a generation not yet dialectically sophisticated—was essentially static. Synthesis tended to shift the emphasis in Renaissance studies from process, on which the traditional estimate of the Renaissance depended, to structure or, minimally, from the long-range processes which gave European history a larger narrative shape to particular, ostensibly self-contained (and in this sense inconsequential), more limited processes. This tendency was supplemented by an influence from another direction: our supposedly innocent but in fact deeply insidious course catalogues. We should treat the course catalogue with more respect. Partly because we are inclined to take it so lightly, it is one of the most potent forces in historiography: it tends to organize the past, for the sake of "coverage," as a sequence of chronologically bounded segments, the number of which reflects the size of our departments. The individual historian is then made responsible for one of these segments, with the expectation that he will deal with it in all of its aspects. And the assignment defined for him by the catalogue, when he is young and malleable, is likely to shape his general

⁵ Randolph Starn has called attention to this; see his review of Nicolai Rubinstein, ed., *Florentine Studies: Politics and Society in Renaissance Italy* (London, 1968), in *Bibliothèque d'Humanisme et Renaissance*, 32 (1970): 682–83. Also see his "Historians and 'Crisis,'" *Past & Present*, no. 52 (1971): 19.

⁶ For explicit recognition that the term functions chiefly as an administrative convenience, see Brian Pullan, *A History of Early Renaissance Italy from the Mid-Thirteenth to the Mid-Fifteenth Century* (London, 1973), 11.

⁷ Mattingly, "Some Revisions of the Political History of the Renaissance," in Helton, *The Renaissance: A Reconsideration*, 3.

understanding of what it means to “do” history.⁸ Thus, the influence of the catalogue has various consequences, among which the most positive is to deepen the historian’s sense of complexity. But the catalogue also discourages him from intruding into adjacent segments that “belong” to his colleagues; and by the same token it encourages him, however conscious he may be of the arbitrariness of the dates bounding his assignment, to treat his segment as self-contained. At the very least, he feels compelled out of esthetic motives to portray it as some kind of intelligible unity.⁹

Historians of the Renaissance have responded to these pressures in two ways. First, we began to distinguish more and more clearly between “the Renaissance” itself, a cluster of cultural movements pregnant with the future, and the “age of the Renaissance,” the more general context within which we encountered these movements. The “age of the Renaissance” was invoked to accommodate in some unstable tension with the novelty and modernity of Renaissance culture whatever seemed inconsistent or in tension with it. But we tended at first to regard these anomalies as so many medieval residues, destined to yield ineluctably, in the long run, to its modernizing forces. This approach was hardly the method of synthesis.

But at the same time we were increasingly uncomfortable with the rather mechanical work of sorting our data into two heaps, one marked “continuities,” the other “innovations.” This discomfort led to a second move that seems on the surface to have brought us closer to synthesis: we began to describe the age of the Renaissance as the age of transition *to* the modern world. And this formula, which now appears with some regularity in our textbooks, has provoked little dissent. Indeed, the formula appears to exclude the possibility of dissent, for it is nicely calculated to accommodate every anomaly and at the same time to protect the significance of the Renaissance. This, of course, is its purpose. To the objection that every past age might equally be represented as transitional, we can reply that this one was *unusually* transitional, that it was an age of *accelerated* transition.¹⁰ This position now gives a semblance of agreement to Renaissance scholarship, enabling us to engage in a wide variety of tasks, comfortable in the belief that our larger claims are secure—and effectively indifferent to them.

Nevertheless, there are difficulties in this apparently unexceptionable strategy. For one thing, it neglects to state the criteria by which one age can be considered more transitional than another; by begging this question, which

⁸ The effect of this periodization by course sequences has doubtless been intensified by the decline of introductory surveys of European history.

⁹ There may be analogies here with the consequences of specialization in other occupations, notably medicine.

¹⁰ In his *Renaissance in Historical Thought*, Ferguson tied the notion of transition to synthesis; he combined the two strategies in *Europe in Transition, 1300–1520* (Boston, 1962), the first large-scale presentation of the period in these terms, though this project was already foreshadowed in his “The Interpretation of the Renaissance: Suggestions for a Synthesis,” *Journal of the History of Ideas*, 12 (1951): 483–95. For other works that rely on the idea of transition, see Eugene F. Rice, Jr., *The Foundations of Early Modern Europe, 1460–1559* (New York, 1970), ix; Lewis W. Spitz, *The Renaissance and Reformation Movements* (Chicago, 1971), vii, 3; and Pullan, *Early Renaissance Italy*, 11. The widespread assumption that textbooks such as these are no part of our “serious” work seems to me both troubling and mistaken.

was at the heart of our controversy with the medievalists, it invites a new revolt from that direction as well as protests from other quarters. The strategy also seems to me conceptually confused, a reflection of the chronic temptation of the historian to identify "history" as the actuality of the past with "history" as the construction he makes of its records. For history as actuality, an "age" is simply a considerable span of time; for history as construction, an "age" is a segment of the past on which he can impose some intelligibility. The notion of an "age of transition" thus exploits what is essentially a structural conception to assert for the Renaissance a continuing significance that actually derives from its place in a process.

This confusion points to a further problem, since the notion of a transitional age depends on the intelligibility of the "ages" it supposedly connects. The Renaissance as "transition" suggests something like an unsteady bridge between two granitic headlands, clearly identifiable as the Middle Ages and the modern (or, at least, the early modern) world. As a Renaissance specialist, I am reluctant to commit myself about the present stability of these two adjacent historiographical promontories. But my impression is that neither medieval nor early modern historians would be altogether comfortable with the image.¹¹ And as an inhabitant of the modern world, I find it rather too amorphous, unintelligible, and contradictory, at least as a whole, to provide any stable mooring for such a bridge. I am, in short, doubtful whether we are yet in any position to represent our own time as an intelligible age.

But a reflection of this kind takes us beyond internal historiographical pressures to the impact of contemporary experience on historiography. And such experience may, in the end, be the major cause for the present disarray of Renaissance scholarship: since we are baffled by the modern world, we are hardly in a position to argue for the relevance to it, at least in the traditional way, of the Renaissance.¹² For the argument that attached the Renaissance to the modern world was based on two assumptions: that the modern world does, in fact, constitute some kind of intelligible entity, and that modernity has emerged by way of a single linear process. Neither of these assumptions is, at least for me, self-evident. To be a competent historian of the Renaissance is, of course, hard enough, even without engaging in extracurricular ventures of this kind; but my efforts to sample the work of those scholars who have struggled to define the modern condition leave me as uncertain as the modern world itself.¹³ And I am further bewildered by the suggestion that we have now entered into a "postmodern" age. Meanwhile, the collapse of the idea of progress has profoundly subverted our sense of the direction of history. We can agree, perhaps, only that the present is the complex product of a remarkably tangled past.

¹¹ It may be noted that medievalists who write about the Renaissance tend to see it not as a "transition" but as having a distinct identity of its own. See, for example, Denys Hay, *The Italian Renaissance in Its Historical Background* (Cambridge, 1961), 14-25; and Robert S. Lopez, *The Three Ages of the Italian Renaissance* (Charlottesville, N.C., 1970), 73.

¹² For a work that is especially sensitive to this problem, see Rice, *Foundations of Early Modern Europe*, x.

¹³ I have been helped to see the complexity of this problem by Richard D. Brown's work; see his *Modernization: The Transformation of American Life, 1600-1865* (New York, 1976), 3-22.

Other pressures from the surrounding world have also weakened the ability of the historian of the Renaissance to defend the old dramatic organization of Western history and have at the same time promoted an alternative. Brought into focus by the social and cultural ferment of the 1960s, so stimulating to historiography in other areas, these pressures have left the Renaissance in a partial eclipse. They pose a radical challenge—one that we have largely ignored—to our own doubtful compromise between process and structure.¹⁴

This challenge is related to a generous concern with the historiographically neglected and suffering majority of mankind that has diverted attention from those elites whose achievements have been the mainstay of claims for the Renaissance. From this standpoint historical significance tends to be defined largely as a function of numbers, of mass, and, hence, of the masses; this interest in the masses may suggest an ideological and even sentimental content in the supposedly cold and scientific impulse toward quantification. But mass also suggests matter and, therefore, points to the material basis of human existence, with a concomitant tendency to rely on the architectural model—so disruptive of traditional historiography—of superstructure and infrastructure, against the idealism often implicit in the preoccupation of historians of the Renaissance with high culture. A further consequence of this interest has been an emphasis on the more inert aspects of the past, with reduced attention to what had traditionally been seen as the source of the most dynamic forces in modern history. Meanwhile, the peculiar insecurity of the last two decades seems to have intensified the occasional yearning of the historian to regard himself as a scientist; and the methods recently devised to promote this aspiration and to open up new social groups to investigation have not been suited to the ways of Renaissance study, which has depended chiefly on the cultivated judgment and creative imagination of the individual historian.

THESE IMPULSES HAVE CONSPICUOUSLY BEEN AT WORK in the new social history, which has produced results of great interest, if chiefly for a later period, and which seems to me itself a remarkable feat of the historical imagination. This much is, I think, indisputable, however skeptical one may be of its scientific pretensions¹⁵ and of the claims of some of its practitioners to have overcome at last the distinction between history as actuality and history as construction. And it is particularly instructive from the standpoint of our present difficulties with the Renaissance, because it displays the results of a deliberate and wholehearted acceptance of that notion of an “age” with which the historian

¹⁴ For a stimulating exception, see John Hale, *Renaissance Europe: The Individual and Society, 1480–1520* (London, 1971). But its short time-span excuses it from the need to deal with larger processes, and in spite of Hale's attempt to write “majority” history, much of his detail is drawn—inevitably—from “minority” sources.

¹⁵ This issue is muddled by the ambiguity of the term “science.” For a useful discussion of its somewhat different meanings in French and English usage, see J. H. Hexter, “Fernand Braudel and the *Monde Braudellien* . . .,” *Journal of Modern History*, 44 (1972): 500.

of the Renaissance has dealt so gingerly. It may also help to explain why he has preferred compromise.

I am referring to the concept of the *longue durée*, the intelligible age *par excellence*, whose implications for the Renaissance emerge with special clarity in a recent essay by Emmanuel Le Roy Ladurie.¹⁶ This piece offers a general interpretation of the extended period between about the eleventh and the nineteenth century. Situated between two intervals of innovation and expansion, this true age is, for Le Roy Ladurie, an intelligible unity, given fundamental coherence by a kind of grim Malthusian balance. The productivity of agriculture was limited, population was limited by it, and the material conditions of life for the vast majority were virtually unchangeable. By the democratic criterion of numbers, this long period was, except in insignificant detail, changeless; Le Roy Ladurie has accordingly described it as "motionless."

From this standpoint the period of the Renaissance appears as little more than, in a double sense, the dead center of a much longer age in which the conventional distinction between medieval and early modern Europe has been obliterated. At most, the Renaissance is a *conjoncture* that is intelligible only in a far larger temporal context. But the full implications of the argument emerge only in Le Roy Ladurie's reply to the objections that might be raised against it by more traditional historians:

One might object to this conception of motionless history . . . because it is a little too negligent of such fundamental innovations of the period as Pascal's divine revelation, Papin's steam engine and the growth of a very great city like Paris, or the progress of civility among the upper classes as symbolized by the introduction of the dinner fork. Far be it from me to question the radically new character of these episodes. But what interests me is the *becoming*, or rather the *non-becoming* of the faceless mass of people. The accomplishments of the elite are situated on a higher and more isolated plane and are not really significant except from the point of view of a noisy minority, carriers of progress without doubt, but as yet incapable of mobilizing the enormous mass of rural humanity enmeshed in its Ricardian feedback.¹⁷

One has only to substitute—for Pascal, Papin, Paris, and the dinner fork—any random set of Renaissance accomplishments—Petrarch's historical consciousness, the Copernican Revolution, the Florentine city-state with its civic rhetoric, and double-entry bookkeeping, for example—to appreciate the morbid implications here for the Renaissance.

Although the plausibility of this argument, which appears to illustrate the consequences of a thoroughgoing "synthesis," has perhaps been one element in the present disarray of Renaissance historiography, its approach also has limitations (as I am hardly the first to point out¹⁸) that make it less decisive for

¹⁶ Le Roy Ladurie, "L'histoire immobile," *Annales: Economies, sociétés, civilisations*, 29 (1974): 673–82, translated by John Day as "Motionless History," *Social Science History*, 1 (1977): 115–36. Clyde Griffen kindly called this article to my attention.

¹⁷ Le Roy Ladurie, "Motionless History," 133–34.

¹⁸ For a notable critique, see Hexter, "Fernand Braudel and the *Monde Braudellien*," 480–539. Also see, for a criticism of the neglect of process in much of the new social history, Eugene and Elizabeth Fox Genovese,

the Renaissance than it may first appear. Largely an adaptation of French structuralism, Le Roy Ladurie's thesis carries with it the antihistorical bias of that movement: structuralist analysis of the past has never been well adapted to deal with change. The consequences are apparent when Le Roy Ladurie, too good a historian to ignore this problem, must account for the end of his *longue durée*, when motion was finally restored to human affairs, the constraints on agriculture loosened, the old Malthusian cycle was broken, the migration from field to factory could begin, and the masses were at last expelled from the traditional world into, presumably, a new age.

At this point Le Roy Ladurie's rich ironies seem to serve chiefly as a rhetorical justification for the limitation of his vision to what, as he so disarmingly puts it, "interests" him. Here we become aware of a difference both in strategy and tone. Since the masses were helpless to bring about this ambiguous denouement, that ridiculous noisy minority becomes unexpectedly important. Now it represents "forces of elitist renovation which had been building up slowly over the course of centuries" and which finally succeeded, after about 1720, in "setting off an avalanche."¹⁹ This "build-up of forces" might suggest that Paris and the steam engine—and even, more obscurely, Pascal and the fork—are after all, if one is interested in that "avalanche," worth some attention. And back of them lies the Renaissance—not, perhaps, as an "age" but (in the terms of its traditional interpretation) as a critical moment in a process that would in the long run significantly transform the world. The impulses not altogether arbitrarily associated with the Renaissance—its individualism and its practical and empirical rationality—were, though immediately limited to a statistically insignificant minority, destined for some importance even from the standpoint of the majority.²⁰ I do not mean to deny the value of structural description; indeed, it provides essential safeguards against anachronism for the historian primarily interested in process.²¹ But structures can hardly exhaust the concern of the historian; the past is not simply a world we have lost.

The inability of a history of structures to deal with change has, however, a further consequence. Its neglect of the continuities that link the past with the present and one "age" to the next opens the way to an interpretation of

"The Political Crisis of Social History: A Marxian Perspective," *Journal of Social History*, 10 (1976): 215. As Robert M. Berdahl points out, many non-Marxists can agree with this; see his "Anthropology and History: A Note and an Example," *Geschichte und Gesellschaft* (forthcoming).

¹⁹ Le Roy Ladurie, "Motionless History," 134.

²⁰ The long-range significance of these tendencies of the Renaissance is still recognized, however, in some recent work. See Jean Delumeau, "Le développement de l'esprit d'organisation et de la pensée méthodique dans la mentalité occidentale à l'époque de la Renaissance," in Thirteenth International Congress of Historical Sciences, Moscow 1970, *Doklady Kongressa*, 1, pt. 5 (Moscow, 1973): 139–50; and Peter Burke, *Culture and Society in Renaissance Italy, 1420–1540* (London, 1972), 225.

²¹ The very real danger of anachronism seems to have led Charles Trinkaus to renounce the "traditional genetic-modernist bias," i.e., the scrutiny of the past in the interest of understanding the present; Trinkaus, "Humanism, Religion, Society: Concepts and Motivations of Some Recent Studies," *Renaissance Quarterly*, 29 (1976): 677, 685–86. Though I agree that it is subject to abuse, I see nothing illegitimate in principle in genetic explanation, and I am quite sure that its abandonment by historians would only leave it to others less sensitive to its difficulties.

change as cataclysm, with the implication that the modern world is genetically related to the past only remotely. Our own time thus appears as something like a biological mutation, whose survival value remains an open question. For the structural approach to the past may ignore but cannot, after all, repudiate process altogether. One set of structures obviously does, somehow, give way to another. The effect of this approach is to promote, however inadvertently, a discontinuous concept of process. Thus, for the myth of continuity with the Renaissance it substitutes what I will call the myth of apocalyptic modernization. In calling this a myth, I mean nothing pejorative.²² A myth is, for the historian, the dynamic equivalent of a model in the social sciences, and we can hardly do without it. The crucial transition from chronicle to history depended on the application of some principle of mythical organization to previously discrete data: the myth of the hero, the myth of collective advance, the myth of decline. That the weakening of one mythical pattern should have left a kind of vacuum for another myth to fill is hardly surprising.

So the apocalyptic myth—a product partly of our own self-importance and partly of the mingled hopes and anxieties generated by recent experience—has emerged, though it is not itself peculiarly modern. A modification of the basic Western myth of linear time of a type periodically recurrent under conditions of stress, the apocalyptic myth provides an alternative to the idea of continuous development, with which it can be variously combined. Indeed, it is not altogether different from the Renaissance notion of radical discontinuity with the Middle Ages. In discussing it critically, I am aware of a certain analogy with the medievalists' protests against the idea of the Renaissance.

Largely, as a result of those protests, historians of the Renaissance generally gave up the apocalyptic dimension of the original Renaissance myth, at least as it related to the past. Without renouncing the novelties of the Renaissance, they recognized its continuities with the Middle Ages, themselves increasingly seen as complex. In other words, they made distinctions, within both periods, among contrary tendencies. But these careful distinctions took care of only half of the Renaissance problem. Thus, if we are still in disarray, the explanation may ultimately be that we have failed to modify in the same way that element in the Renaissance myth that pointed to the future: its perception of the modern world—the goal of the historical process—as a coherent entity. Since we can no longer support our claims for the Renaissance origins of the modern world so conceived, we have fallen silent. If this is true, the full solution to the Renaissance problem would thus depend on our giving as much attention to the complexities and contradictions of our own time as we have given to those of the Middle Ages and the Renaissance and on being equally selective about the relation of the Renaissance to the modern world. Among its other advantages, this solution might enable us to put the apoc-

²² For this complex word, see Raymond Williams, *Keywords: A Vocabulary of Culture and Society* (New York, 1976), 176–78. For a generally instructive work on the role of myth in historiography, see Hayden White, *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (Baltimore, 1973).

alyptic myth itself in some perspective; we might then notice that some reaction against it is already under way in the social sciences.²³

Such selectivity might enable us to claim for the Renaissance a substantial role in the formation of those tendencies in our own world that perhaps have a better claim to modernity than does the present apocalyptic mood: the skeptical, relativistic, and pragmatic strains in contemporary culture.²⁴ These strains would suggest, in place of the apocalyptic myth, something like the myth of Prometheus, itself of some interest to Renaissance thought²⁵—Prometheus who, by tricking Zeus and stealing the fire that made possible the arts, endowed man with the power to create a world in which he could survive alone. Such a myth might be interpreted to mean that the world man inhabits is formed, not through some transcendent and ineluctable process—whether cataclysmic or uniform—but only out of his own shifting needs and unpredictable inventiveness. From this standpoint, the basic peculiarity of the modern world might be seen as the present consciousness of human beings of their power to shape the world they inhabit, including the social world and, by extension, themselves. A (for us) poignant reflection of this situation might be the unique predicament of the modern historian, who is in a position to choose, among various possibilities, the myth most useful to impose dramatic organization on his data—a problem of which previous historians were largely unaware. In modern culture, then, the determinism and helplessness implicit in the apocalyptic myth are opposed by a still lively belief in human freedom.

The modern sense of the creative freedom of mankind now finds stimulating expression in a concept of culture that underlies the work of a group of distinguished contemporary anthropologists.²⁶ According to this view of the human condition, the universe man inhabits is essentially a complex of meanings of his own devising; man, as Max Weber perceived him, is “an animal suspended in webs of significance he himself has spun.”²⁷ These webs make up his culture or, more exactly, since they are utterly various, his cultures. Furthermore, as philosophers and linguists have made increasingly clear, he spins these webs from language. Through language man orders the chaos of data impinging on his sensorium from, in a singularly mysterious and problematic sense, “out there,” organizing them into categories and so mak-

²³ See Shmuel N. Eisenstadt, “Sociological Theory and an Analysis of the Dynamics of Civilizations and Revolutions,” *Daedalus*, 106 (1977): esp. 61–63.

²⁴ Isaiah Berlin has helped me bring these strains into focus; see his *Vico and Herder: Two Studies in the History of Ideas* (New York, 1976).

²⁵ See Charles Trinkaus, *In Our Image and Likeness: Humanity and Divinity in Italian Humanist Thought*, 1 (Chicago, 1970): 244–45. Also see, for a significant and more recent application of this myth, Donald R. Kelley, “The Metaphysics of Law: An Essay on the Very Young Marx,” *AHR*, 83 (1978): 350.

²⁶ For studies that reflect this concept of culture, see Pierre Bourdieu, *Outline of a Theory of Practice*, trans. Richard Nice (Cambridge, 1977); Mary Douglas, *Purity and Danger: An Analysis of Concepts of Pollution and Taboo* (London, 1966), *Natural Symbols* (London, 1970), and *Implicit Meanings: Essays in Anthropology* (London, 1975); Louis Dumont, *From Mandeville to Marx: The Genesis and Triumph of Economic Ideology* (Chicago, 1977); Clifford Geertz, *The Interpretation of Cultures* (New York, 1973); Marshall Sahlins, *Culture and Practical Reason* (Chicago, 1976); Victor Turner, *The Ritual Process: Structure and Anti-Structure* (Ithaca, N.Y., 1969); and, seminal for the role of language in culture, Edward Sapir, *Culture, Language, and Personality: Selected Essays*, ed. David G. Mandelbaum (Berkeley and Los Angeles, 1949).

²⁷ Geertz, *Interpretation of Cultures*, 5.

ing them intelligible, manageable, and useful. The human world might, therefore, be described as a vast rhetorical production, for the operations that bring it into existence are comparable to such basic rhetorical transactions as division and comparison, or metonymy and metaphor.²⁸ This concept denies not that an objective universe exists but only that man has direct access to it or can know what it is apart from what he makes of it, out of his own limited perceptual and intellectual resources and for his own purposes, whatever these might be.²⁹

The epistemological decisions embedded in language are thus the precondition of human apprehension of an external world; culture in this sense is prior to both materialism and idealism, which represent contrary efforts to assign ontological status to—in the language of sociology, to legitimize—a world whose actual source in the creativity of man violates the all-too-human need for transcendence.³⁰ From this standpoint history presents itself not as a single process but as a complex of processes, which interests us insofar as we are interested in the almost infinite possibilities of human existence. Beyond this, history as construction often tends to be a misleading and sometimes pernicious reification.

HERE, I AM ONLY ADVANCING ON AN OLD POSITION in the historiography of the Renaissance from a somewhat new direction. For the kind of history this approach suggests was very much that of the most distinguished historians of the Renaissance of the last hundred years, Jacob Burckhardt and Johann Huizinga, notable pioneers in what both called cultural history. Misled by their concentration on evidence drawn from the culture of elites, we have tended to see in their work no more than the study of “superstructure,” losing sight of the generous conception of culture underlying their work. For Burckhardt, the proper subject of *Kulturgeschichte* was not simply the arts, which were relatively neglected in his account of the Renaissance, but “what moves the world and what is of penetrating influence, . . . the indispensable.”³¹ For Huizinga, cultural history required the identification of “deeper, general themes” and “the patterns of life, thought, and art taken all together,” which he was prepared to pursue in every dimension of human experience.³² And both had such reservations about the modern world that neither would have found much satisfaction in representing it as the goal of history.

This conception of culture is perhaps the contemporary world’s most gen-

²⁸ The historian’s creation of the world of the past out of language provides a close analogy.

²⁹ For much of this I am indebted to the theoretical essays of Harry Berger, Jr. See, in particular, his “Outline of a General Theory of Cultural Change,” *Clio*, 2 (1972): 49–63, and “Naive Consciousness,” *Papers on Language and Literature*, 8 (1973): 1–44.

³⁰ See Sahlins, *Culture and Practical Reason*, esp. ix–x.

³¹ As quoted in Karl J. Weintraub, *Visions of Culture: Voltaire, Guizot, Burckhardt, Lamprecht, Huizinga, Ortega y Gasset* (Chicago, 1966), 138.

³² Huizinga, *Men and Ideas: History, the Middle Ages, and the Renaissance*, trans. James S. Holmes and Hans van Marle (New York, 1959), 28. Also see Weintraub, *Visions of Culture*, 230–31.

eral legacy from the Renaissance: the recognition that culture is a product of the creative adjustment of the human race to its varying historical circumstances rather than a function of universal and changeless nature, and the perception that culture accordingly differs from time to time and group to group. This insight of the Renaissance suggested that mankind, by its own initiatives, could, for better or worse, shape its own earthly condition. Hints of this idea can be found earlier, of course, both in antiquity and in the Middle Ages; and even in the Renaissance the idea was limited to certain groups in which it only occasionally became explicit—as it did for Petrarch and Nicholas of Cusa (though only at certain moments), for Sir Philip Sidney, and for Montaigne. But this shocking view of the human condition made its first durable impression on the Western consciousness then and has continued to shape our world.

The high culture of the Renaissance immediately revealed some of the implications of the new conception of culture. Scholars became aware of the distinct, historically contingent cultures of antiquity, while the voyages of exploration discovered the varieties of contemporary culture in America and the Orient. Although the first European responses to these revelations tended to be ethnocentric, the relativism of Montaigne suggested that another kind of reaction was already possible. Meanwhile, cultural expression was being conceived, more modestly, not as a total and authoritative reflection of external reality but as a particular human insight, conveyed by isolated proverbs, *pensées*, familiar essays, small areas of practical or esthetic order, of which the autonomous painting of Renaissance art provides a nice symbol.

Perhaps the most profound indication that a radical shift in the understanding of culture was taking place—and, hence, a shift in the sense of man's relation to the world and to himself—can be seen in the Renaissance crisis of language, that basic instrument in the formation of culture.³³ The first sign of that crisis was a growing uneasiness, at first among the most abstract thinkers but then more broadly, that the human vocabulary was failing to mirror the objective world. *Words*, it was widely lamented, no longer corresponded to *things*. This lament was often taken to mean that the vocabulary should be reformed so that this traditional identity could be restored: a demand, in effect, for a return to the dependence of culture upon external nature. But then an alternative solution to the problem began to unfold. Skepticism about the capacity of the human mind to grasp the structures of nature directly led

³³ For a general discussion of Renaissance views of language, see Karl-Otto Apel, *Die Idee der Sprache in der Tradition des Humanismus von Dante bis Vico*, Archiv für Begriffsgeschichte, no. 8 (Bonn, 1963). For some of the studies that have influenced my own understanding of these matters, see Michael Baxandall, *Giotto and the Orators: Humanist Observers of Painting in Italy and the Discovery of Pictorial Composition, 1350–1450* (Oxford, 1971); Salvatore I. Camporeale, *Lorenzo Valla: Umanesimo e teologia* (Florence, 1972); Thomas M. Greene, "Petrarch and the Humanist Hermeneutic," in K. Atchity and G. Rimanelli, eds., *Italian Literature: Roots and Branches* (New Haven, 1976), 201–24; Gordon Leff, *William of Ockham: The Metamorphosis of Scholastic Discourse* (Manchester, 1975), esp. 124–237; J. G. A. Pocock, *Politics, Language, and Time: Essays on Political Thought and History* (New York, 1971); and Nancy S. Struener, *The Language of History in the Renaissance: Rhetoric and Historical Consciousness in Florentine Humanism* (Princeton, 1970). It is increasingly apparent that those self-conscious antagonists, Renaissance humanists and later Scholastics, in fact collaborated in this development.

to growing doubt about the possibility of such an identity, to a recognition of the conventionality of language and its susceptibility to change, to the perception of language as a human creation, and eventually to the conclusion that, as the creator of language, man also shapes through language the only world he can know directly, including even himself.

This insight was a major impulse behind the brilliant imaginative literature of the Renaissance, which was one channel for the diffusion of this new concept of language. So was the steady displacement of Latin, the language of absolute truths both sacred and profane, by the European vernaculars, not only in literature but in law and administration. The variety of the vernaculars suggested that language was based on the consensus of particular peoples, arrived at by the processes of history; and the growing expressiveness of the various languages of Europe appeared to demonstrate that linguistic change signified not that the primordial identity of language with the real world was being corrupted—the traditional view propounded by Socrates in the *Cratylus*—but that language is a flexible tool. The rich elaboration of vernacular languages was not only the deliberate project of elites but a spontaneous and increasingly popular eruption to meet the shifting requirements of existence.

There was thus nothing ethereal about this portentous cultural shift. If a common culture is the foundation of community and limits the possible modes of social organization and social action, it is also responsive to changing social needs, themselves culturally defined. And, like other historical phenomena, the subtle and reciprocal dialogue between culture and society is open to investigation.³⁴ The expanding linguistic resources of Renaissance culture simultaneously facilitated and reflected the development of a more complex urban and monarchical society. The sense that language does not simply mirror, passively, the structures of external nature but functions as a tool to serve the practical needs of social existence eventually stimulated reflection about the uses and creative possibilities of language. And we can see in those reflections the germ of a new vision of human culture.

Whether given practical expression in the creative modification of language or, at another level, in the Renaissance idea of self-fashioning,³⁵ the notion of man as creator of himself and the world was heady stuff. It found expression in the modern expectation that government, the economy, and education should constantly reconstruct society, the environment, and man himself in accordance with the constantly changing expectations of mankind. There are doubtless limits to such an enterprise, both in the malleability of physical and

³⁴ For an especially useful discussion of this relationship, see Bourdieu, *Outline of a Theory of Practice*, esp. 72–95.

³⁵ On this radical application of the Renaissance concept of human creativity, see A. Bartlett Giamatti, "Proteus Unbound: Some Versions of the Sea God in the Renaissance," in Peter Demetz, ed., *The Disciplines of Criticism* (New Haven, 1968), 431–75; and Stephen J. Greenblatt, "Marlowe and Renaissance Self-Fashioning," in Alvin Kernan, ed., *Two Renaissance Mythmakers: Christopher Marlowe and Ben Jonson* (Baltimore, 1977), 41–69.

biological reality and in man's own moral capacities,³⁶ that this aspiration tends to overlook. These limits and the attempts to exceed them help to explain a perennial impulse since the Renaissance to react against the creativity and freedom of Renaissance culture toward various types of philosophical and scientific determinism and, thus, also to explain the contradictions of the modern world. Perhaps the Renaissance vision of man with its vast practical consequences has needed, from time to time, to be chastened in this way. But it has so far survived as the major resource with which to oppose the temptation to escape from the anxieties of the human condition into new versions of authoritarianism.

I BEGAN THESE REMARKS by announcing the collapse of the dramatic scheme that has long organized our vision of the general career of Western history. Since I think that drama is vital to historiography, because it enables us to impose form on the processes of history and so to make them intelligible, this seems to me an ominous development, especially since it has invited the substitution of another dramatic scheme that would deprive us of our roots in the past. But, although I have argued for the continuing significance of the Renaissance, I have not tried simply to defend the traditional pattern, which seems to me seriously defective, in ways that the legacy of Renaissance culture also helps us understand. The old dramatic pattern, with its concept of linear history moving the human race ineluctably to its goal in the modern world depended on concealed principles of transcendence inappropriate to the human understanding of human affairs. The trinity of acts composing the great drama of human history and its concept of the modern epoch as not just the latest but the last act of the play bear witness to its eschatological origins,³⁷ and such notions seem to me peculiarly inappropriate to so human an enterprise as that of the historian. But I also find the traditional scheme unsatisfactory because it is not dramatic enough. It fails to accommodate the sense of contingency and, therefore, suspense—the sense that the drama might have turned out otherwise—that belongs to all human temporal experience. Though it has survived for over five centuries, for example, I see no reason to assume that the anthropological vision we owe to the Renaissance is destined to triumph forever over the forces arrayed against it, and much in the modern world suggests the contrary.

But the more human concept of the drama of history that had its effective origins in the Renaissance understanding of culture overcomes these various disadvantages. Its pluralism implies the possibility of a multiplicity of historical dramas, both simultaneous and successive; and so it relieves us of the

³⁶ Hence, the condemnation of the Renaissance in Protestant neo-orthodoxy; see Herbert Weisinger, "The Attack on the Renaissance in Theology Today," *Studies in the Renaissance*, 2 (1955): 176-89. This hostility continues to inhibit recognition of the filiation between the Reformation and the Renaissance.

³⁷ The structural principle of the conventional ancient-medieval-modern division seems to persist in more recent trinitarian schemes—i.e., primitive-traditional-modern and aristocrat-bourgeois-proletarian.

embarrassment, inherent in a linear and eschatological vision of time, of repeatedly having to reclassify in other terms what for a previous generation seemed modern. Since it perceives history as a part of culture and also, therefore, a human creation, it permits us constantly to reconstruct the dramas of history and so to see the past in fresh relationships to ourselves. Above all, since it insists on no particular outcome for the dramas of history, it leaves the future open.

Images of Power: Art and Pageantry in Renaissance Venice

EDWARD MUIR

IN THE VOLUPTUOUS SERENITY of the reclining or sleeping female nudes so popular in Renaissance Venetian painting, Millard Meiss saw a reflection of Venice's reputation for political and social concord. Florence's dynamic activity was represented, in contrast, by male figures, often those of David, that struggle and aspire.¹ For Meiss these two artistic proclivities expressed more than differing cultural preferences; they were metaphors for the divergent *Weltanschauungen* of the two cities and for their dissimilar political experiences: Venice was politically harmonious; Florence was contentious.

As is well known, the city republics of late medieval and Renaissance Italy were turbulent places—faction-ridden, prone to erecting *ad hoc* institutions, liable to bring in *signore* to solve emergencies, and accustomed to the sort of fighting in the streets immortalized in *Romeo and Juliet*. In comparison, the Republic of Venice, *La Serenissima*, seemed secure and politically unified under the rule of a hereditary patriciate, whose members styled themselves nobles. Venice's constitutional arrangements lasted with few changes from the late thirteenth century to 1797, an extraordinary continuity that, coupled with Venice's widespread if often unfounded reputation for unsullied liberty, unwavering religiosity, social harmony, and unfailingly peaceful intentions, has come to be called—and studied as—the “myth of Venice.”² Longevity of

This article began to take shape when Felix Gilbert asked me to prepare a paper to be read at a Renaissance Society of America Conference on “Art and Politics” held at Smith College in March 1976. At the invitation of Christopher Lloyd, I read a greatly altered version before the Seminar on Aspects of the Early Renaissance in Italy at the Ashmolean Museum, Oxford, in June 1977. I am indebted to these gentlemen, to D. R. Edward Wright and Donald Weinstein, who saved me from many blunders, and to the National Endowment for the Humanities for a summer grant in 1977 that supported additional research.

¹ Meiss, “Sleep in Venice: Ancient Myths and Renaissance Proclivities,” *Proceedings of the American Philosophical Society*, 110 (1966): 348–62.

² For the rapidly growing literature on the “myth of Venice” and on Venetian republican thought, see, among others, Jacob Burckhardt, *The Civilization of the Renaissance in Italy*, trans. S. C. G. Middlemore, 2 vols. (reprint ed., New York, 1958), 1: 82–95, 2: 420–22; Angel A. Castellan, “Venecia como modelo de ordenamiento político en el pensamiento italiano de los siglos XV y XVI,” *Anales de historia antigua y medieval*, 12 (1963–65): 7–42; Federico Chabod, “Venezia nella politica italiana ed europea del Cinquecento,” in *La civiltà veneziana del Rinascimento* (Florence, 1958), 27–55; Gaetano Cozzi, “Cultura politica e religione nella ‘pubblica storiografia’ veneziana del ‘500,” *Bollettino dell’ Istituto di Storia della Società e dello Stato Veneziano*, 5–6 (1963–64): 215–94; Gina Fasoli, “Nascita di un mito,” in *Studi storici in onore di Gioacchino*

institutions does not mean, of course, that social and political conflict was absent in Venice. Yet, whatever conflict there was occurred within a political context of such unusual stability that, from the sixteenth to eighteenth centuries, Venice enjoyed fame in both Europe and America as the ideal example of the virtue of a republic of laws in an age of increasingly absolutist monarchies.³

Volpe (Florence, 1958), 445–79; Franco Gaeta, “Alcuni considerazioni sul mito di Venezia,” *Bibliothèque d’Humanisme et Renaissance*, 23 (1961): 58–75; Felix Gilbert, “Biondo, Sabellico, and the Beginnings of Venetian Official Historiography,” in J. G. Rowe and W. H. Stockdale, eds., *Florentine Historians: Essays Presented to Wallace K. Ferguson* (Toronto, 1971), 275–93, “Religion and Politics in the Thought of Gasparo Contarini,” in Theodore K. Rabb and Jerrold E. Seigel, eds., *Action and Conviction in Early Modern Europe: Essays in Memory of E. H. Harbison* (Princeton, 1969), 90–116, “The Date of the Composition of Contarini’s and Giannotti’s Books on Venice,” *Studies in the Renaissance*, 14 (1967): 172–84, and “Venetian Diplomacy before Pavia: From Reality to Myth,” in J. H. Elliott and H. G. Koenigsberger, eds., *The Diversity of History: Essays in Honour of Sir Herbert Butterfield* (London, n.d.), 81–116; Myron P. Gilmore, “Myth and Reality in Venetian Political Theory,” in John Hale, ed., *Renaissance Venice* (London, 1973), 431–43; Paul F. Grendler, “Francesco Sansovino and Italian Popular History, 1560–1600,” *Studies in the Renaissance*, 16 (1969): 139–80; Margaret L. King, “Personal, Domestic, and Republican Values in the Moral Philosophy of Giovanni Caldierra,” *Renaissance Quarterly*, 28 (1975): 535–74; Patricia H. Labalme, *Bernardo Giustiniani: A Venetian of the Quattrocento* (Rome, 1969), 247–304; Frederic C. Lane, “At the Roots of Republicanism,” in *Venice and History: The Collected Papers of F. C. Lane* (Baltimore, 1966), 320–37, “Medieval Political Ideas and the Venetian Constitution,” in *ibid.*, 285–307, and *Venice: A Maritime Republic* (Baltimore, 1973), 220, 256–73, *passim*; Lester J. Libby, “Venetian History and Political Thought after 1509,” *Studies in the Renaissance*, 20 (1973): 7–45; Oliver Logan, *Culture and Society in Venice, 1470–1790: The Renaissance and Its Heritage* (London, 1972), 1–19; Brian Pullan, “The Significance of Venice,” *Bulletin of the John Rylands Library*, 56 (1974): 443–62; Ellen Rosand, “Music in the Myth of Venice,” *Renaissance Quarterly*, 30 (1977): 511–37; and James Bruce Ross, “The Emergence of Gasparo Contarini: A Bibliographical Essay,” *Church History*, 41 (1972): 22–45. And, of course, see the magisterial work of William J. Bouwsma: *Venice and the Defense of Republican Liberty: Renaissance Values in the Age of the Counter Reformation* (Berkeley and Los Angeles, 1968). The idea of civic myth as developed by Venetian historians has been applied to the history of other cities as well. See, for example, Aldo Garosci, “La formazione del mito di San Marino,” *Rivista storica italiana*, 71 (1959): 21–47; Donald Weinstein, *Savonarola and Florence: Prophecy and Patriotism in the Renaissance* (Princeton, 1970), 27–66; and Werner L. Gundersheimer, *Ferrara: The Style of a Renaissance Despotism* (Princeton, 1973), 278–81.

³ On the reputation of the republic outside of Venice, see Bronisław Biliński, “Venezia nelle peregrinazioni polacche del Cinquecento e lo Sposalizio del Mare di Giovanni Siemuszowski (1565),” in Mieczysław Brahmner, ed., *Italia, Venezia, e Polonia tra umanismo e rinascimento* (Warsaw, 1967), 233–90; William J. Bouwsma, “Venice and the Political Education of Europe,” in Hale, *Renaissance Venice*, 445–66; Horatio F. Brown, “Shakespeare and Venice,” in his *Studies in the History of Venice*, 2 (New York, 1907): 159–80, and “Cromwell and the Venetian Republic,” in *ibid.*, 296–321; Zera S. Fink, “Venice and English Political Thought in the Seventeenth Century,” *Modern Philosophy*, 38 (1940–41): 155–72, and *The Classical Republicans: An Essay in the Recovery of a Pattern of Thought in Seventeenth-Century England* (2d ed., Evanston, Ill., 1962), 28–51; Elio Gianturco, “Bodin’s Conception of the Venetian Constitution and His Critical Rift with Fabio Alberghati,” *Revue de littérature comparée*, 18 (1938): 684–95; Felix Gilbert, “The Venetian Constitution in Florentine Political Thought,” in Nicolai Rubinstein, ed., *Florentine Studies: Politics and Society in Renaissance Florence* (Evanston, 1968), 463–500, and “Machiavelli e Venezia,” *Lettere italiane*, 21 (1969): 389–98; J. R. Hale, *England and the Italian Renaissance: The Growth of Interest in Its History and Art* (London, 1954), 29–32; Karol Koranyi, “La costituzione di Venezia nel pensiero politico della Polonia,” in Brahmner, *Italia, Venezia, e Polonia tra umanismo e rinascimento*, 206–14; John Leon Lievsay, *Venetian Phoenix: Paolo Sarpi and Some of His English Friends (1606–1700)* (Lawrence, Kans., 1973); Nicola Matteucci, “Machiavelli, Harrington, Montesquieu e gli ‘ordini’ di Venezia,” *Il pensiero politico*, 3 (1970): 337–69; Renzo Pecchioli, “Il mito di Venezia e la crisi fiorentina intorno al 1500,” *Studi storici*, 3 (1962): 451–92; Richard H. Parkinson, “‘Volpone’ and the Reputation of Venetian Justice,” *Modern Language Review*, 35 (1940): 11–18; J. G. A. Pocock, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, 1975), 183–330 *passim*; and Charles Jerome Rose, “The Evolution of the Image of Venice (1500–1630)” (Ph.D. dissertation, Columbia University, 1971). For William Penn’s interest in Venice, see H. F. Russell Smith, *Harrington and His Oceana* (Cambridge, 1914), 171. For evidence that social and political conditions in Venice did not often agree with the mythical image of the city, see Stanley Chojnacki, “Crime, Punishment, and the Trecento Venetian State,” in Lauro Martines, ed., *Violence and Civil Disorder in Italian Cities, 1200–1500* (Berkeley and Los Angeles, 1972), 184–228, and “In Search of the Venetian Patriarchate: Families and Factions in the Fourteenth Century,” in Hale, *Renaissance Venice*, 47–90; Gaetano Cozzi, “Domenico Morosini e il ‘De bene instituta re publica,’” *Studi veneziani*, 12 (1970): 405–58, “Authority and the Law in

Venice was also a city that gloried in fine appearances and outward show; in fact, the face of things was so valued that the marble- and relief-bedecked facades on the canal side of many palaces still contrast startlingly with the less public parts of unadorned brick. Many of the cultural preoccupations of the Renaissance, especially the humanists' emphasis on rhetorical grace and the Neoplatonic belief that outward beauty was a sign of inward virtue, encouraged the cultivation of appearances and often the neglect or suppression of what modern historians would consider the underlying realities. Even Machiavelli, so effective in unmasking reputations, advised those who would govern that "the great majority of mankind are satisfied with appearances, as though they were realities, and are often even more influenced by things that seem than by those that are."⁴ Thus, the capacity of the visual arts to convey beguiling impressions entranced Renaissance rulers, grave republican patri-cians and princely dandies alike, and artistic patronage became an arm of government. The Venetian republic appears to have been the most successful Italian example of a government that recognized and adapted to its own purpose the political potential of the arts, and, as Meiss's work has shown, to attempt to decipher a political code in Venetian art is not new.

From the twelfth to fourteenth centuries many Italian city-republics used the arts to teach political virtues. One of the best examples is Ambrogio Lorenzetti's *Allegory of Good and Bad Government*, painted in circa 1338-39 for the Palazzo Pubblico in Siena. A major theme in this fresco is civic concord—an elusive condition in Siena—illustrated by two figures that dominate the work: one is a personification of Justice and the other of "Good Government," represented by a sceptred and crowned emperor or ruler, flanked by the virtues. The two figures are joined by a cord that passes through the hand of a female figure, labeled "Concordia," and that is grasped by a dignified group of twenty-four citizen magistrates who march in procession to pay homage to "Good Government."⁵ The fresco posits peace, harmony, and security as the benefits enjoyed by citizens who obey the laws and ignore the temptations of ambition and avarice to support communal government. Using an artistic

Renaissance Venice," in Hale, *Renaissance Venice*, 311-14, and *Il Doge Nicolò Contarini: Ricerche sul patriziato veneziano agli inizi del Seicento* (Venice, 1958), 229-83; Lane, *Venice: A Maritime Republic*, 258-65; Roland Mousnier, "Le trafic des offices à Venise," *Revue historique de droit français et étranger*, 4th ser., 30 (1952): 552-66; Donald E. Queller, *Early Venetian Legislation on Ambassadors* (Geneva, 1966), "The Civic Irresponsibility of the Venetian Nobility," in David Herlihy, Robert L. Lopez, and Vsevolod Slessarev, eds., *Economy, Society, and Government in Medieval Italy: Essays in Memory of Robert L. Reynolds* (Kent, Ohio, 1969), 223-36, and "The Development of Ambassadorial *Relazioni*," in Hale, *Renaissance Venice*, 174; Charles J. Rose, "Marc Antonio Venier, Renier Zeno, and 'The Myth of Venice,'" *Historian*, 36 (1974): 479-97; Guido Ruggiero, "Sexual Criminality in Early Renaissance Venice, 1338-58," *Journal of Social History*, 8 (1975): 18-37; and Alberto Tenenti, "The Sense of Space and Time in the Venetian World of the Fifteenth and Sixteenth Centuries," in Hale, *Renaissance Venice*, 19.

⁴ Machiavelli, *Discourses on Livy*, 1. 25, as translated in Luigi Ricci, ed., *The Prince and the Discourses* (New York, 1950), 182. Machiavelli also recommended that the prince patronize the arts and sciences and entertain the people on occasion with festivals and shows; *The Prince*, chaps. 18, 19. Elsewhere, Machiavelli cited an example of the Medici sponsoring public festivals in 1466 as a way of diverting attention from an unpopular policy; *The History of Florence*, 7. 12.

⁵ Nicolai Rubinstein, "Political Ideas in Siennese Art: The Frescoes by Ambrogio Lorenzetti and Taddeo di Bartolo in the Palazzo Pubblico," *Journal of the Warburg and Courtauld Institutes*, 21 (1958): 179-207; and Hélène Wieruszowski, "Art and the Commune in the Time of Dante," *Speculum*, 19 (1944): 14-33.

theme current at the time of Dante, Lorenzetti emphasized communal responsibility rather than the privileges of a dictatorial prince or a governing elite.

Bernard Berenson argued three-quarters of a century ago that by the time of the Renaissance the Venetian government alone remained true to the medieval civic tradition exemplified by the Lorenzetti fresco—that the Venetian state, unlike other Italian cities, avoided neoclassical allegories too complex for the simple citizen and encouraged painting “as did the church, in order to teach its subjects its own glory in a way that they could understand without being led on to critical inquiry.”⁶ In his emphasis on the continuity of the medieval artistic traditions in Venice, the didactic concerns of the government, and the Venetian aversion to elitist distinctions, Berenson touched on what remains one of the engaging enigmas of the Venetian Renaissance: that is, what of historical value can be learned from the public image a government painted of itself? Can one, without being seduced by the propaganda, learn something about the society that produced it by studying the forms the propaganda took? To answer these questions, there are three possible approaches: (1) an examination of the relationship between political ideas and motifs in the arts; (2) a study of the men who patronized the arts; and, in light of Berenson’s claim for continuity, (3) a search for changes and mutations in the iconographic tradition.

IN THE POLITICAL ARTISTIC TRADITION OF VENICE some works of art justified Venetian conquests of foreign territories and proclaimed Venice a “New Rome”; others inspired belief in the civic cult constructed around the legendary history of the city, the hagiography of Saint Mark, and the adoration of the Virgin Mary; and still others testified to the attempts of an individual or family within the supposedly egalitarian patriciate to use art to enhance personal prestige or influence. In order best to trace the interplay of tradition, political conditions, and social change in shaping Venetian art, one may turn to the decorations of the great governmental buildings and ceremonial spaces at San Marco and to the curious rise in the sixteenth century of the political art forms of tableaux vivants and pageantry.⁷

No building in Venice was more splendidly embellished or richer in political intent than the Basilica of San Marco, at the heart of the republic. Built as a martyrium, apostolic church, private chapel, and state sanctuary, San Marco was the focus of civic pride and the ritual seat of government.⁸ In fact,

⁶ Berenson, *The Italian Painters of the Renaissance* (rev. ed., 1894; reprint ed., Cleveland, 1957), 16.

⁷ On the proclivity of Italian cities in particular to transform large areas of public space into a festive stage, see André Chastel, “Le lieu de la fête,” in Jean Jacquot, ed., *Les fêtes de la Renaissance*, 1 (Paris, 1956): 419–23.

⁸ Otto Demus, *The Church of San Marco in Venice: History, Architecture, Sculpture*, Dumbarton Oaks Studies, no. 6 (Washington, 1960), 67. On the *jus patronato* of the doges over the basilica, legally the private chapel of the doges, see Staale Sinding-Larsen, *Christ in the Council Hall: Studies in the Religious Iconography of the Venetian Republic*, Institutum Romanum Norvegiae, Acta ad archaeologiam et artium historiam pertinentiae, no. 5 (Rome, 1974), 180. The visual subordination of San Marco to the political establishment was such that the orientation of the ducal throne and other seats in the chapel choir made the basilica appear to be an extension of the Council Halls of the Ducal Palace; see Bruce Boucher, “Jacopo Sansovino and the Choir of St. Mark’s,” *Burlington Magazine*, 118 (1976): 564.

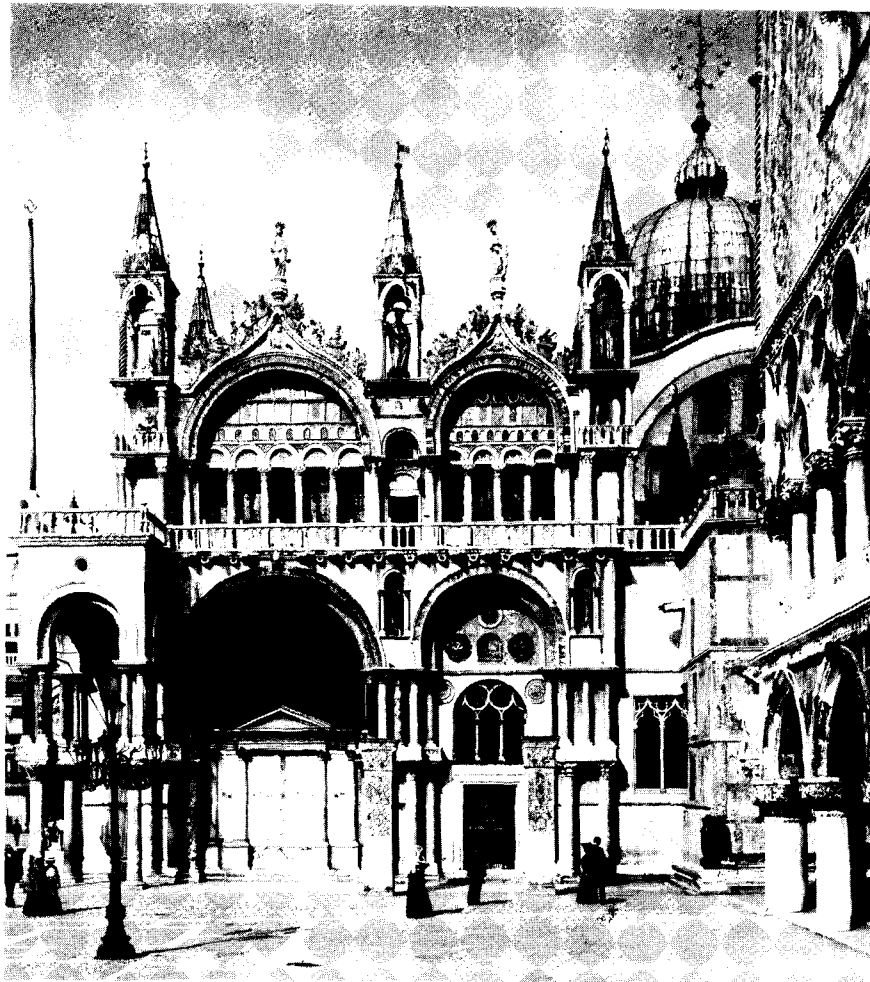


Figure 1: Basilica di San Marco, south facade. Photograph courtesy of Alinari.

as Otto Demus has shown, “the political point of view had such great importance for Venetians that every detail of the decoration of their state church could, and did, take on a political aspect.”⁹ Many of the most important decorations were added after 1204 when their conquest of Constantinople prompted the Venetians to picture themselves as the rebuilders of the Apostolic Empire of the East, the true heirs of the Christian Empire of Constantine and Justinian, as opposed to the misguided pretenders, the Comneni in Byzantium and the Hohenstaufen in Germany.¹⁰

To legitimize their new empire, the Venetians adopted two complementary policies, both of which found expression in art. One policy was the arrangement of symbols of sovereignty around San Marco to portray the idea that the conquered territories were now under the rule of Saint Mark and that the

⁹ Demus, *The Church of San Marco*, 54.

¹⁰ Otto Demus, “A Renaissance of Early Christian Art in Thirteenth-Century Venice,” in Kurt Weitzmann, ed., *Late Classical and Medieval Studies in Honor of Albert Mathias Friend, Jr.* (Princeton, 1955), 356–57.

administration of divine justice had become the temporal duty of an imperial Venetian state. For instance, on the south facade, the seaward approach to the basilica, three symbols of Genoese colonial government were prominently displayed: two free-standing pillars and the short platform from which Genoese decrees had been read, the *pietra del bando*, now affirmed the transfer of jurisdiction to Venice. On the wall of the treasury adjacent to these captured symbols of justice were placed statues of the ancient Roman tetrarchs, reliefs, columns, and capitals taken from Constantinople.¹¹ These governmental insignia, in effect, claimed for Venice the right of succession to the *imperium* of Rome. Such decorations gave the seaward approach, and what was before the sixteenth century the principal ceremonial entrance to San Marco, a political and triumphant character. On the west facade of the basilica, the famous bronze horses—once adornments of a Roman triumphal arch, now also booty of eastern campaigns—reiterated the connection between Venice's spiritual and imperial authority. Thus, the works of art so carefully assembled at San Marco at once justified Venice's colonizing adventures and gilded rapine with the legal symbols and artistic glories of other great empires.

The second policy followed by thirteenth-century Venetians to sanction their new position was the fabrication of a pseudo-history of their city. In one such effort, Dominicans, probably under the direction of the doge, revised the hagiographical tradition of Saint Mark to show that since apostolic times Venice had been destined to be not just a shrine for the relics of Saint Mark but the seat of his authority as well. Certain works of art reveal other early political attitudes toward Venetian history and iconography. For example, although Venetian art was profoundly influenced stylistically by Greek artists brought to Venice after 1204, imitating Greek models did not mean abandoning Venetian control over the symbols and images in commissioned works of art. To illustrate, on the west facade of San Marco two of the four rectangular reliefs on either side of the central porch depict seated warriors about to unsheathe or replace their swords. Otto Demus has shown that one figure is a Greek work of the late twelfth or early thirteenth century of Saint Demetrius, probably brought to Venice as part of the spoils of Constantinople. The other figure, identified in an inscription as Saint George, is a less graceful copy of the Saint Demetrius by a Venetian artist.

Since the thoughtful arrangement of the other artistic loot of 1204 shows a concern for political messages, this pairing of warrior saints is also significant: to pair Saint Demetrius with Saint George rather than with Saint Theodore, who had been the traditional military protector of Venice, constituted an iconographical shift that can be explained by the political situation of the early thirteenth century. Saint Theodore had been an early symbol of Venice's dependence on the Byzantine Empire, and the gradual secession of Venice from the control of Byzantium paralleled Theodore's almost total

¹¹ Demus, *The Church of San Marco*, 113.

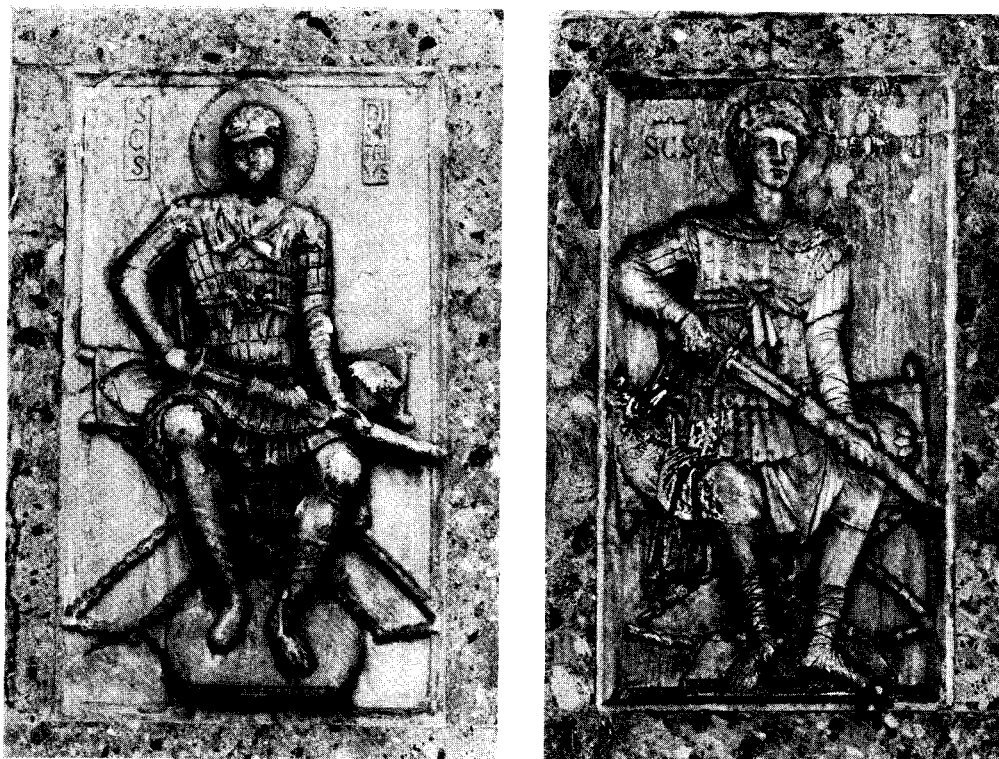


Figure 2: Marble reliefs of Saint Demetrius and Saint George, west facade of the Basilica di San Marco. Photographs courtesy of Alinari.

eclipse in the city. At a time when Venice had just conquered the capital of its former master, the depiction of Saint Theodore on the facade of the Venetian state church would have been singularly inappropriate, even though in Eastern Christendom he was the usual partner for Saint Demetrius.¹² Thus, when the decorative scheme for the facade of San Marco called for the pairing of a military protector saint with the Saint Demetrius brought from Constantinople, the Venetians entirely erased their history as a formerly subordinate power and replaced their outmoded protector with Saint George, whose relics they proudly possessed. The city's imperial ambitions demanded a coordinate iconography. Through such calculated artistic choices, the Basilica of San Marco became an exceptionally political as well as religious edifice, a monument not of popular piety, but of political religiosity. As Demus has remarked, "it was shaped not so much by impersonal and largely unconscious trends and sentiments, as by the conscious will of a ruling caste, whose representatives wanted it to be the visible symbol and programmatic embodiment of their ideas."¹³

In the fourteenth century Western influence further encroached upon Byzantine ideas in the Venetian imagination. In their struggle against heresy, for

¹² *Ibid.*, 126–35.

¹³ *Ibid.*, 59. In 1450 the Senate revived the cult of Saint Theodore when it declared a *festum solemnis* for his feast day; Biblioteca del Museo Correr, Venice [hereafter MCV], Cod. Cicogna 2043, f. 31. Other evidence

example, the mendicant orders in Venice promoted new iconographic types, although not necessarily a new style: the Coronation of the Virgin and other Marian lore became the friars' favored themes. Since the Venetian government did not allow them the freedom of action they enjoyed elsewhere in Italy, the Dominicans and Franciscans were obliged to preach their cause largely through the visual arts.¹⁴ The Marian cult served the orders well, and, as a result of the increased popularity of images of the Virgin in the artistic repertoire of Venice, the government pressed these motifs into the service of the domestic political iconography. In particular, the crowned Virgin came to personify an inviolate, transcendent Venice. At the same time, the Venetians made their first major territorial conquests on the Italian mainland and under the influence of Western ideas began to portray the city as a "New Rome," modeled on the pagan Empire of the Caesars rather than on the Apostolic Empire of Byzantium as it had been in the previous century.¹⁵ By the fifteenth century rudiments of the Renaissance image of Venice had appeared: Byzantine artistic motifs bowed to Western ones; in addition to Saint Mark, the Virgin now became enshrined as a symbol of Venice; and, while the Byzantine conception of a mystical/religious city-state survived, the moribund glory of imperial Byzantium fell before the more commanding legacy of Rome.

The Venetian creation of a civic cult around the achievements of a distinguished past is evident in the ornamentation of the Ducal Palace. Although many of the palace's early art treasures were lost in a series of disastrous fires, huge canvases on historical and allegorical subjects, much like the late sixteenth-century ones that today dominate the vast halls of the palace, must have served to exhort earlier generations of aristocratic magistrates to emulate the virtues of their forefathers.¹⁶ There were at least three such series. In 1574 and 1577 the second and most intriguing set of paintings in the Great Council Hall burned. In them Carpaccio, Gentile and Giovanni Bellini, and Titian,

for the revival of the cult includes the promotion in 1552 of the Scuola di San Teodoro, founded in the thirteenth century, to the status of a Scuola Grande; Brian Pullan, *Rich and Poor in Renaissance Venice: The Social Institutions of a Catholic State to 1620* (Oxford, 1971), 59.

¹⁴ Galienne Francastel, "Une peinture anti-hérétique à Venise?" *Annales: Économies, Sociétés, Civilisations*, 20 (1965): 1-17.

¹⁵ On the theme of Venice as a new Rome, see D. S. Chambers, *The Imperial Age of Venice, 1380-1580* (London, 1970), 12-30. Frederic Lane has referred to the change as the "turn westward"; Lane, *Venice: A Maritime Republic*, 202-72. On the theme of Venice as a new Rome in the arts, also see Deborah Howard, *Jacopo Sansovino: Architecture and Patronage in Renaissance Venice* (New Haven, 1975), 2-7; and Michelangelo Muraro, "La scala senza giganti," in Millard Meiss, ed., *Essays in Honor of Erwin Panofsky* (New York, 1961), 350-70. Also see William Hammer, "The Concept of the New or Second Rome in the Middle Ages," *Speculum*, 19 (1944): 50-63; and Robert Lee Wolff, "The Three Romes: The Migration of an Ideology and the Making of an Autocrat," *Daedalus*, 88 (1959): 291-311.

¹⁶ On the medieval art in the Ducal Palace, see Sergio Bettini, "Appunti di storia della pittura veneta nel medioevo," *Arte veneta*, 20 (1966): 20-42 and 21 (1967): 21-33; Mirella Levi d'Ancona, "Giustino del fu Gherardino da Forlì e gli affreschi del Guariento al Palazzo Ducale di Venezia," *Arte veneta*, 21 (1967): 34-44; and B. Rackham, "A Note on the Symbolism of Some Sculptures of the Ducal Palace, Venice," *Journal of the Warburg and Courtauld Institutes*, 23 (1960): 308-09. On the Ducal Palace in general, see Michelangelo Muraro, "Venezia: Interpretazione del Palazzo Ducale," *Studi urbinati di storia, filosofia, e letteratura*, 45 (1971): 1160-93; and Sinding-Larsen, *Christ in the Council Hall*, *passim*. On the artistic theme of comparing the

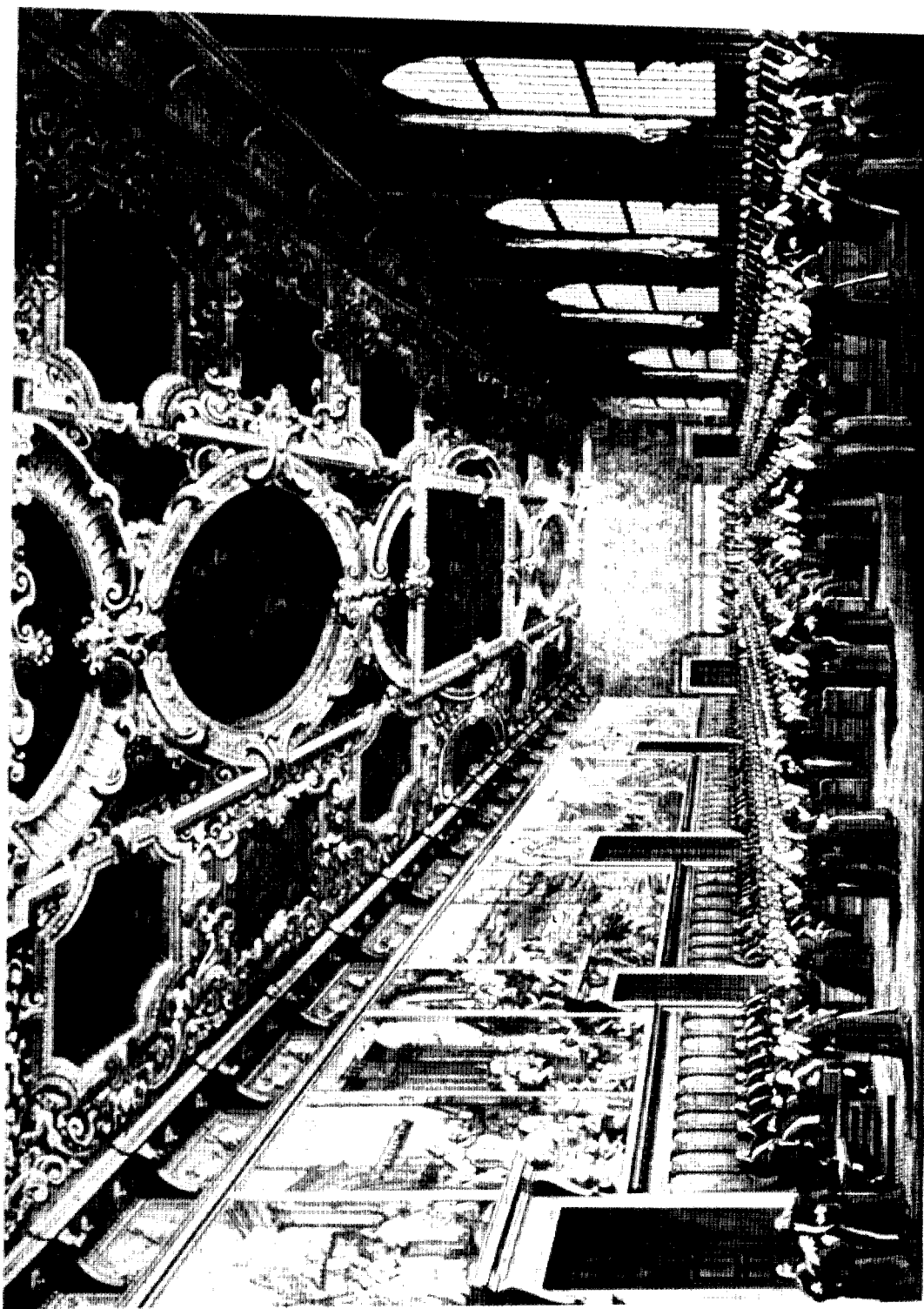


Figure 3: Antonio Canal, painter, and G. B. Brustolon, engraver, "The Great Council Hall." Photograph courtesy of Museo Civico Correr, Venice.

among others, had depicted Venetian military victories, especially those of the Crusades, and the city's legendary rescue of Pope Alexander III from the threats of Emperor Frederick Barbarossa. For example, in a surviving sketch for one of Carpaccio's paintings, Pope Alexander gives a monarch's umbrella or baldachin to Doge Sebastiano Ziani; according to the Venetian tradition, this honor made him a third Christian potentate, equal to the emperor and the pope. In describing the Bellinis' contributions to the Ducal Palace, Vasari said that the governing patricians commissioned these paintings because they thought that "here should be depicted the glory and magnificence of their most admirable city, her deeds in war, her most important undertakings, and all things worthy of being remembered by all posterity, in order that to the pleasure of reading history should be added the gratification of the intellect in seeing so many nobles and their exploits illustrated."¹⁷

After the great fires of the 1570s, a special commission of nobles drew up the program for the redecoration of the Hall of the Great Council and the adjacent Hall of the Scrutinio. A cycle of historical paintings followed the earlier scheme, but the commission replaced the Marian paintings with Christ and the "Last Judgement," suggesting that through obedience to God's chosen government in Venice one could attain salvation. Venice was no longer personified by the Virgin but by an enthroned Queen, who in Veronese's *Apotheosis of Venice* is crowned with a laurel wreath by a figure representing "Victory."¹⁸ That there had been some distinct changes from the simple communal ideal espoused by such works as Lorenzetti's fresco *Good Government* is increasingly clear.

Changes in Venetian political art may in part be attributed to the factional and personal conflicts inherent in any republic. Often individual ambition endangered the fine balance of power among officials; and, just as the decorations of the Ducal Palace illustrate the communal ideas and mythical history of the republic, they also display the remnants of the thwarted designs of once powerful men. The man who might pose the greatest challenge to the communal idea was, of course, the doge: on the one hand, an elected official with severely circumscribed powers and, on the other, a mystical embodiment

Ducal Palace to the Palace of Solomon, see Sinding-Larsen, *Christ in the Council Hall*, 169–70; and David Rosand, "Titian's Presentation of the Virgin in the Temple and the Scuola della Carità," *Art Bulletin*, 58 (1976): 78.

¹⁷ Vasari, *Lives of the Artists*, trans. John Howard Smith (New York, 1946), 131. Vasari, *Le vite de' più eccellenti pittori scultori e architettori*, 3 (Novara, 1967): 87–88: "... nelle quali si dipignessero le conorate magnificenze della loro maravigliosa città, le grandezze, le cose fatte in guerra, l'imprese et altre cose somiglianti, degne di essere rappresentate in pittura alla memoria di coloro che venissero; acciochè all'utile e piacere che si trae dalle storie che si leggono, si aggiugnesse trattenimento all'occhio et all'intelletto parimente, nel vedere da dottissima mano fatte l'imagini di tanti illustri signori, e l'opere egregie di tanti gentiluomini, dignissimi d'eterna fama e memoria."

¹⁸ For a description of the cycle, see Girolamo Bardi, *Dichiaratione di tutte le istorie, che si contengono ne i quadri posti novamente nelle Sale dello Scrutinio, & del Gran Consiglio, del Palazzo Ducale della Serenissima Repubblica di Vinegia* . . . (Venice, 1587). For the program for the decorations written in 1579–80 by Giacomo Marcello and Giacomo Contarini with the aid of Girolamo Bardi, see MCV, Cod. Cicogna D1XXXV–105, published in Wolfgang Wolters, "Der Programmwurf zur Dekoration des Dogenpalastes nach dem Brand vom 20 Dezember 1577," *Mitteilungen des Kunsthistorischen Instituts in Florenz*, 12 (1966): 271–318. For discussions of the iconography of these paintings, see Charles de Tolnay, "Il 'Paradiso' del Tintoretto note sull'interpretazione della tela in Palazzo Ducale," *Arte veneta*, 24 (1970): 103–10; and Sinding-Larsen, *Christ in the Council Hall*, 23, 29, 43, 220–64.

of the authority of Saint Mark, "as splendid as the sun." The very ambiguity of his constitutional authority augured political conflict.¹⁹ In recognition of this fact, memorials to individual patricians were legally excluded from San Marco, and tradition frowned on any exuberant artistic celebration of individual doges in the Ducal Palace. No doges were buried in San Marco after 1354,



Figure 4: Vittorio Carpaccio, "Pope Alexander III Bestows a Ceremonial Parasol on Doge Sebastiano Ziani at Ancona," pen and bistre ink drawing. Photograph courtesy of E. B. Crocker Gallery, Sacramento.

and expressions of the power or personal tastes of a doge were usually confined to his family's private palace or to a tomb in the Dominican church, Santi Giovanni e Paolo, distant from the governmental center at San Marco.²⁰ And the standard iconography used to depict a doge showed him humbly

¹⁹ Giovanni Caldiera compared the doge to the sun; King, "Personal, Domestic, and Republican Values," 564–68. For the conventional view of the doge, see Gasparo Contarini, *De magistratibus et republica Venetorum libri quinque* (Venice, 1551), 38–43, translated into English by Lewes Lewkenor as *The Commonwealth and Government of Venice* (London, 1599), 37–42. On the ambiguity of the ducal office, see Edward Muir, "The Doge as *Primus Inter Pares*: Interregnum Rites in Early Sixteenth-Century Venice," in Sergio Bertelli and Gloria Ramakus, eds., *Essays Presented to Myron P. Gilmore*, 1 (Florence, 1978): 145–60. For a critical view of the doges dating from the early sixteenth century, see Cozzi, "Domenico Morosini," 405–58.

²⁰ The last doge to be buried in San Marco was Andrea Dandolo; Andrea da Mosto, *I dogi di Venezia nella vita pubblica e privata* (Milan, n.d.), 143–53. On several occasions in the fifteenth and sixteenth centuries, the government refused to authorize the erection of a memorial to any individual in or near San Marco. Despite *condottiere* Bartolomeo Colleoni's bequest of one hundred thousand ducats to fight the Turk in exchange for allowing a bronze statue of him to be erected in Piazza San Marco, the republic placed it elsewhere. Bortolo Belotti, *La vita di Bartolomeo Colleoni* (2d ed., Bergamo, 1933); and Lane, *Venice: A Maritime Republic*, 233. On the attempts of Tommaso Rangone to have a portrait statue of himself placed opposite San Marco, see Howard, *Jacopo Sansovino*, 81. The major exception to this rule was the tomb built in a special chapel in

kneeling in supplication before the lion of Saint Mark.²¹ During the late fifteenth and early sixteenth centuries, however, the accepted restraints on the artistic representations of the doges in the Ducal Palace were repeatedly ignored.

Two monuments in the courtyard of the Ducal Palace reveal the trend toward self-aggrandizement of doges. The first is the Foscari arch, modestly begun under Doge Francesco Foscari (1423–57) but enlarged and made grander by Doge Cristoforo Moro (1462–71). Debra Pincus has argued that the arch's fifteenth-century form with its multiple levels of statuary—including a nude Adam and Eve, a doge kneeling before the lion of Saint Mark, personifications of the seven Liberal Arts, warriors, an *Amor Dei* (a woman holding a flaming bowl), and Saint Mark—created a new ceremonial space, in which the doge figured as the restorer of the earthly paradise lost by Adam and Eve and promised anew by the Gospel of Saint Mark.²² This was certainly a lofty claim for any man who had climbed to the pinnacle of the Venetian *cursus honorum* through the inevitably unholy process of electoral politics.

The second, and even more self-congratulatory, monument, which faces the Foscari arch and completed the ceremonial complex devoted to the doges, is the grandiose marble staircase now called the *Scala dei Giganti*. Commissioned by Doge Marco Barbarigo in 1485 and finished by his brother, Agostino, who succeeded him in office, the reliefs on the side of the staircase magnified the heroic Agostino as the leader of the New Rome. Portraits of Aristotle and Alexander referred to the combination of wisdom and military prowess that characterized the ideal ruler; the coats of arms from enemy cities recalled the victories of Agostino's reign; and allusions to the crafts and guilds indicated popular support for his authority. Lastly, symbols of justice and sovereignty surrounded Doge Agostino's own portrait and, along with the ten inscriptions of his name, claimed for him an elevated, princely status.²³

The Barbarigo doges apparently went too far in offending the egalitarian sentiments of the patriciate. In addition to the staircase, Marco initiated a new, princely coronation ceremony, and Agostino even required visitors to kneel in his presence. The patriciate's legal retaliation was swift and the redress of the political abuse of the arts longlasting: at Agostino's death the

San Marco for Cardinal Giovanni Battista Zen, who died in 1501. For copies of his will ordering the tomb, see Biblioteca Nazionale Marciana, Venice [hereafter BMV], MS italiano VII, 1723 (8598), last folio in the busta; and Marino Sanuto, *I diarii*, eds. Rinaldo Fulin et al., 58 vols. (Venice, 1879–1903), 30: cols. 272–76. Also see MCV, MS Venier P.D. 517b, n.p. (under heading "Maggio"); BMV, MS Latin III, 172 (2276), ff. 77v–79r; Francesco Sansovino, *Venetia città nobilissima et singolare*, with additions by Giovanni Stringa (Venice, 1604), 96, 522–23, 598–99; and Nicolò Doghioni, *Le cose notabile et maravigliose della città di Venetia* (Venice, 1692), 228–30.

²¹ Examples of this motif are innumerable. For the kneeling doge in illuminations of official documents, see Giordano Mariani Canova, "La decorazione dei documenti ufficiali in Venezia," *Atti dell'Istituto Veneto di Scienze, Lettere, ed Arti*, 126 (1967–68): 319–34.

²² Pincus, *The Arco Foscari: The Building of a Triumphant Gateway in Fifteenth-Century Venice*, Outstanding Dissertations in the Fine Arts (New York, 1970), 208–52.

²³ Muraro, "La scala senza giganti," 350–70. On Agostino Barbarigo's other attempts to portray himself as a prince of the new Rome, see Chambers, *The Imperial Age*, 25–26; and Nicolò Trevisan, "Cronaca veneta," BMV, MS italiano VII, 519 (8438), f. 294v (new foliation). Also see Filippo Nani-Mocenigo, "Testamento del Doge Agostino Barbarigo," *Nuovo archivio veneto*, new ser., 17 (1909): 234–61.

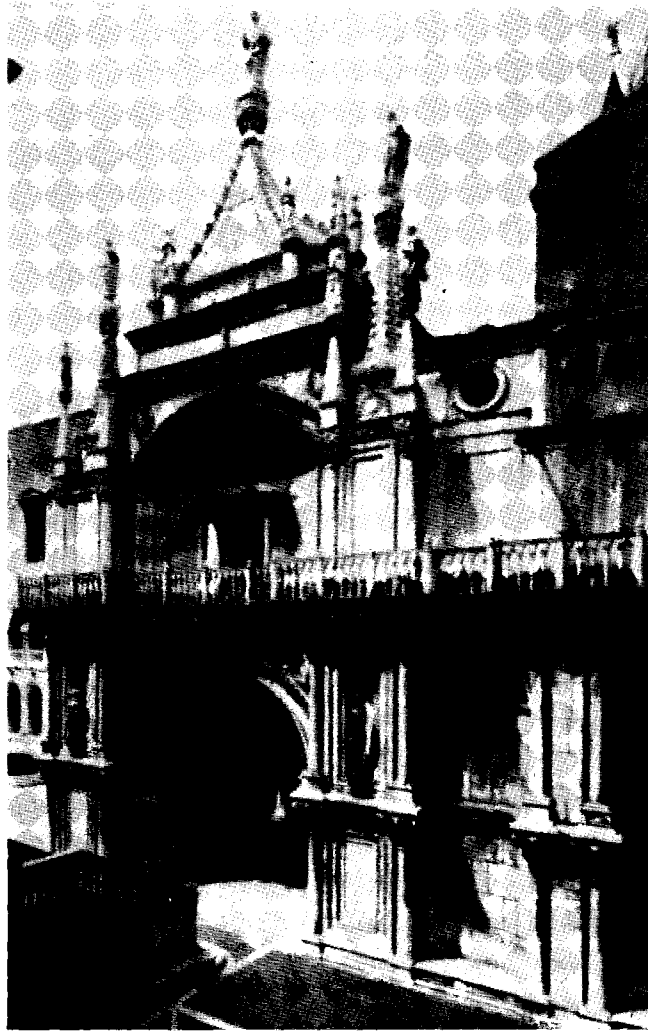


Figure 5: The Arco Foscari, main facade.
Photograph courtesy of Palazzo Ducale, Venice.

Senate passed laws carefully controlling the behavior of future doges. Even as late as 1544, Jacopo Sansovino was commissioned to sculpt two giant marble statues of Mars and Neptune symbolizing the power of the Republic on land and sea; these statues were then mounted at the top of the offending stairs to deflect attention from the Barbarigo imagery and to dwarf future doges who were crowned there.²⁴

The resolution of the conflict between the pretensions of certain doges to princely dignity and the legal status of the doge as merely *primus inter pares*

²⁴ On the Barbarigo doges, see Cozzi, "Authority and the Law in Renaissance Venice," 302. On the legal actions after the Barbarigos' deaths, see Muir, "The Doge as *Primus Inter Pares*," 146–47. On the Mars and Neptune statues, see John Pope-Hennessy, *Italian High Renaissance and Baroque Sculpture* (2d ed., London, 1970), 408; and Howard, *Jacopo Sansovino*, 33. The posture of the doge is reduced on the Foscari arch in a similar fashion by the addition in the keystone of the arch of a winged putto holding a globe of governmental authority and an unrolled scroll; Pincus, *The Arco Foscari*, 383. With only one exception, after 1501 no one appears kneeling in front of a doge in the illuminations for the *capitolari* of the government; Sinding-Larsen, *Christ in the Council Hall*, 178.

was permanent, if uneasy. Only occasionally did an overly glorious, personal portrayal of a doge ignore the governing consensus. For example, from the time of Leonardo Loredan, who died in 1521, each sixteenth-century doge, except for Antonio Grimani (1521–23), commissioned with his own funds a votive portrait for the Ducal Palace. Normally, these portraits avoided con-



Figure 6: Scala dei Giganti.
Photograph by the author.

trovery by posing the doge as a humble servant of God, kneeling before the Virgin or Christ, attended by Saint Mark and the doge's patron saint.²⁵ But the portrait of Doge Sebastiano Venier (1577–78), which originally pictured him as a great hero solely responsible for the Venetian victory over the Turks at Lepanto, assumed too much in expressing the greatness of an individual at the expense of the other Venetians who had participated in the battle. Staale Sinding-Larsen has pointed out that in Francesco Sansovino's description of the chiaroscuro sketch for Veronese's allegory, Venice, dressed as a beautiful queen, crowned Sebastiano Venier as doge and Saint Mark appeared in the heavens looking down. But the painting as it now exists was altered sometime after Doge Venier's death: the government ordered that the Venice-as-Queen figure be relegated to the background and replaced with Saint Justine, on whose feast day the victory had occurred, and that the figure of Saint Mark in

²⁵ Sinding-Larsen, *Christ in the Council Hall*, 24–30.

the heavens be exchanged for that of Christ. Mark was repainted standing at Doge Venier's side, and another hero of the battle, Agostino Barbarigo (not to be confused with the doge of the same name), was added to the scene. The alteration reduced the stature of Doge Venier from that of a ruler chosen by Saint Mark and crowned by the city to that of one hero among many who, with the aid of Saint Mark, had led the city to victory. Veronese's votive picture caused trouble not only because it raised Sebastiano Venier too high but because it ignored a distinction, which had gained acceptance in the sixteenth century, between the mortal doge elected by his peers and the eternal, mystical office of the doge.²⁶ As long as the representations of the two conceptions of doges were kept separate in ceremony and the arts, the threat of an overly ambitious doge to the public image of Venice as a constitutional republic was minimal. When the separation was blurred, as in the case of the Barbarigo stairs or the original for Doge Venier's votive, there were many cautious patricians who raised the alarm.

From the second quarter of the sixteenth century, a more insidious subversion of the republic's egalitarian traditions emerged at the hands of an oligarchy, which was ultimately far more successful than any doge in manipulating the apparatus of government to its own advantage. Whereas a doge's use of artistic imagery for personal advantage had been obvious and therefore could be contained, the oligarchs used the arts more subtly, emphasizing the authority of the state, the exclusiveness of the nobility, and the duties of all citizens to obey their rulers. One cannot find the same kind of direct links between the political pretensions of the oligarchs and the works of art they commissioned as one can in the case of the doges. The evidence is rather more elusive, involving the successful suppression of ducal pretensions at the same time that obedience to other authorities was emphasized, the calculated planning of pageantry to divert the viewers' interests to foreign concerns, and the careful control of the iconographical programs in all of the arts created for official and governmental patrons. And, most cunningly, the elimination of the opportunity for political advancement for the majority of the nobles was accompanied by the elevation in the arts of the exclusiveness and distinctiveness of the nobility as a class.

DURING THE SIXTEENTH CENTURY members of the Venetian ruling class witnessed a number of events that could only lead them to question the efficacy of their traditional values. The results of this questioning and re-evaluating included a greater division within the patriciate, a more rigid authoritarianism, an abandonment of trade by the nobility in favor of investments in

²⁶ Sinding-Larsen, "The Changes in the Iconography and Composition of Veronese's Allegory of the Battle of Lepanto in the Doge's Palace," *Journal of the Warburg and Courtauld Institutes*, 19 (1956): 298-302, and *Christ in the Council Hall*, 84-98. On the artistic representation of two kinds of doges, see De Tolnay, "Il 'Paradiso' del Tintoretto," 104. On the ritual representation of two kinds of doges, the mortal doge and his immortal office, see Muir, "The Doge as *Primus Inter Pares*." Also see Ernst H. Kantorowicz, *The King's Two Bodies: A Study in Medieval Political Theology* (Princeton, 1957), *passim*.

land, and an anxiety to display a noble lifestyle through conspicuous consumption. News of two events arrived in Venice in 1499, precipitating the malaise: one report lamented that the Turks had defeated the Venetian fleet at Zonchio and the other, a letter from Alexandria, warned that three ships under the Portuguese flag had arrived in Aden and Calicut in search of spices. With the naval defeat the Venetians lost their claims to many Greek and Albanian cities, vital to the eastern trade of *La Serenissima*, and, according to Frederick Lane, Venice's leaders henceforth abandoned their commitment to naval supremacy.²⁷ Although it was a century or more before Venice experienced a permanent economic loss as a result of the Portuguese adventure, the startling news that it had been outmaneuvered in the campaign to secure the lucrative pepper trade did not buttress domestic confidence in the shipping business.²⁸ But events much closer to home triggered the deepest paroxysm of insecurity. On May 14, 1509 the combined forces of France, Spain, the Empire, the papacy, and various Italian states under the banner of the League of Cambrai routed the entire Venetian army at Agnadello. Soon most of Venice's mainland domain from Brescia to Padua declared for the enemy, and Venice prepared for a siege that never came.²⁹ Divisions within the league and seven years of war saved Venice and returned most of its former territories, but many Venetians would never recapture the faith that *La Serenissima* could rule a great empire or be the mistress of the seas, however much the gigantic statues of Mars and Neptune on the stairs of the Ducal Palace might proclaim the contrary.

Felix Gilbert and Gaetano Cozzi have shown that in the decades after the war there were deep and important structural changes in the patriciate. Gilbert has discovered a division between the majority of nobles in the Great Council and a small, wealthy elite from the Senate, among whom the most powerful offices were shared and rotated. Their greater wealth and expanded control of the bureaucracy, resulting from the war, allowed these oligarchs to consolidate their power and to ignore the ever more alienated lesser nobles. Cozzi sees a similar shift in the relative power of the two major judicial bodies of the republic: the *avogadoria di comun*, which had guaranteed equality to all nobles, gradually lost its jurisdiction to the Council of Ten, which, through the stringent devices of trial and punishment, meted out justice to support oligarchical authority. The increased emphasis on authority and rigid application of the penal laws indicated to Cozzi that, despite its zeal, the oligarchy's hold on power was indeed precarious.³⁰ In Venice such power

²⁷ Frederic C. Lane, "Naval Actions and Fleet Organization, 1499-1502," in Hale, *Renaissance Venice*, 146-73, and *Venice: A Maritime Republic*, 242.

²⁸ Donald Weinstein, *Ambassador from Venice: Pietro Pasqualigo in Lisbon, 1501* (Minneapolis, 1960), esp. 9-10.

²⁹ Lane, *Venice: A Maritime Republic*, 242-45; Lester J. Libby, Jr., "The Reconquest of Padua in 1509 according to the Diary of Girolamo Priuli," *Renaissance Quarterly*, 28 (1975): 323-31; and Robert Finlay, "Venice, the Po Expedition, and the End of the League of Cambrai, 1509-1510," *Studies in Modern European History and Culture*, 2 (1976): 37-72.

³⁰ Gilbert, "Venice in the Crisis of the League of Cambrai," in Hale, *Renaissance Venice*, 274-92, esp. 290; and Cozzi, "Authority and the Law in Renaissance Venice," esp. 293-95, 205-07, 325-27, 338. Also see Bouwsma, *Venice and the Defense of Republican Liberty*, 95-161. In contrast, Frederic Lane has argued from the

struggles and accompanying structural changes did not, as in many other Italian cities, result in institutional changes, a fact which in the sixteenth century enhanced the republic's reputation for stability and harmony.

The continuing insecurity of the Venetian patriciate was also manifest in the transfer of many families' investments from trade to land. Although Venetian nobles had owned mainland estates from as early as the fourteenth century, the War of the League of Cambrai seems to mark the beginning of their increased acquisition of such holdings.³¹ Economic historians are still debating the reasons for the shift of interests, but the causes do not appear to be entirely or perhaps even primarily economic; despite some violent fluctuations in market conditions, the prosperity of Venice was not in decline, and, in fact, the last half of the century brought economic advances, particularly in industry.³² Patricians bought mainland properties, erected villas, and reclaimed for cultivation vast tracts of land, partially because agricultural investments were more secure than trade and produced an adequate profit but also because the possession of an estate added consequence to the family's social standing.³³ The leisure of a landed income contrasted sharply with the toil and anxiety of the merchant's gain; so it is not strange that the withdrawal from trade was in many cases voluntary. By the beginning of the seventeenth century the demission of trade was so common that Doge Leonardo Donà, who had himself abandoned the ships after 1570, could declare that "the nobility wants no part in trade."³⁴ Even a family like the da Ponte, whose wealth was growing in the sixteenth century, diverted capital from

evidence of dress and intermarriages that the distinctions between noble and commoner were blurred; *Venice: A Maritime Republic*, 253. Although such indifference to class may have been true once, it seems to have gone out of fashion in the sixteenth century. The heightened concern for authority that Cozzi discerned can also be seen in the guild organization of Venetian painters. David Rosand has argued that the failure of Venetian artists to keep up with modern developments in the seventeenth century can be partially blamed on the state's use of the guild to keep tight control of painters; Rosand, "The Crisis of the Venetian Renaissance Tradition," *L'Arte*, 11-12 (1970): 5-6.

³¹ Daniele Beltrami, *Saggio di storia dell'agricoltura nella Repubblica di Venezia durante l'età moderna* (Venice, 1955), and *La penetrazione economica dei veneziani in terraferma: Forze di lavoro e proprietà fondiaria nella campagna veneta dei secoli XVII e XVIII* (Venice, 1961); and S. J. Woolf, "Venice and the Terraferma: Problems of the Change from Commercial to Landed Activities," *Bollettino dell'Istituto di Storia della Società e dello Stato Veneziano*, 4 (1962): 415-41, reprinted in Brian Pullan, ed. *Crisis and Change in the Venetian Economy in the Sixteenth and Seventeenth Centuries* (London, 1968), 175-203, esp. 190-91.

³² Fernand Braudel, "La vita economica di Venezia nel secolo XVI," in *La civiltà veneziana del Rinascimento* (Florence, 1958), 81-102, and *The Mediterranean and the Mediterranean World in the Age of Philip II*, 1 (New York, 1972): 78-81, 290-93, 516-42; Frederic Lane, "Recent Studies on the Economic History of Venice," *Journal of Economic History*, 23 (1963): 312-34; the articles in Pullan, *Crisis and Change in the Venetian Economy*, especially Domenico Sella, "Crisis and Transformation in Venetian Trade," 88-105; and Brian Pullan, "The Occupations and Investments of the Venetian Nobility in the Middle and Late Sixteenth Century," in Hale, *Renaissance Venice*, 38.

³³ "The nobles' insufficient ability to assimilate the new factors which were transforming the nature of mercantile activity revealed them as feeling progressively less like merchants and more like nobles"; Ugo Tucci, "The Psychology of the Venetian Merchant in the Sixteenth Century," in Hale, *Renaissance Venice*, 358. Also see Alberto Tenenti, "The Sense of Space and Time in the Venetian World of the Fifteenth and Sixteenth Centuries," in *ibid.*, 21.

³⁴ Quoted in James Cushman Davis, *The Decline of the Venetian Nobility as a Ruling Class*, The Johns Hopkins University Studies in Historical and Political Science, ser. 80, no. 2 (Baltimore, 1962), 40. On Leonardo Donà's change of investment interests, see James Cushman Davis, *A Venetian Family and Its Fortune, 1500-1900: The Donà and the Conservation of Their Wealth*, *Memoirs of the American Philosophical Society*, no. 106 (Philadelphia, 1975), 37-42.

commerce to land in a search for greater security.³⁵ Thus, patricians whose forefathers, including Marco Polo, had molded their renowned prosperity as capitalist adventurers now avoided risks, occupied spring days strolling their estates instead of rigging ships, and fed their silk worms during the summer months their fathers had spent haggling in the bazaars of the Levant.

Besides living on great estates, nobles were supposed to be generous and open-handed, as the saying went, *spendere largamente*. As if in almost desperate compensation for their mercantile heritage and despite ingrained habits of personal frugality, many patricians in the middle and late sixteenth century spent reckless sums on buildings, tombs, entertainments, and other ostentatious items. Contemporaries believed that conspicuous consumption grew as the patriciate retreated from maritime commerce; foreigners were often amazed at the glittering displays they witnessed, and more than one family's slide from wealth to ignominy was blamed on an unreasonable pursuit of pomp.³⁶ There were repeated and apparently futile attempts on the part of the government to limit the amounts the nobles might expend on wedding banquets and pearl necklaces for their daughters; the Great Council even established a special agency, the *Magistrato alle Pompe*, to monitor private consumption.³⁷ The notion that expensive self-indulgence would eat away at the patrimony of the ruling class apparently worried the older, sober men who sat in the inner councils of the state. Yet that same government, as much as it condemned such extravagance for private purposes, enthusiastically encouraged opulent array when it could advance the dignity of the state. Private expenditure to add luster to the republic was approved, indeed invited, and the rulers took pains to keep the grandeur of the state commensurate with the higher standards of the new fashion by advocating, as well, public support of commissions to magnify Venice.

IN THE YEARS AFTER THE WAR OF THE LEAGUE OF CAMBRAI, the person most directly concerned with elevating the *nobiltà* of the republic was Doge Andrea Gritti (1523–38). A polyglot who spoke French, Latin, Greek, and Turkish and a man who behaved in the grand manner of a tyrant, Gritti personally instigated a number of artistic projects.³⁸ The legal restrictions imposed on the doges after the death of Agostino Barbarigo constrained any ambitions Gritti may have had for self-aggrandizement; so he devoted himself to refash-

³⁵ William Archer Brown, "Nicolò da Ponte: The Political Career of a Sixteenth-Century Patrician" (Ph.D. dissertation, New York University, 1974), 30–57.

³⁶ Davis, *The Decline of the Venetian Nobility*, 44–47; and Logan, *Culture and Society in Venice, 1470–1790*, 148–49. For an example of a patrician incurring heavy expenses from his own pocket to ensure the necessary pomp and ceremony while podestà of Choggia, see Pullan, "The Occupations and Investments of the Venetian Nobility," 390. In the seventeenth century the Venetian patriciate's pursuit of conspicuous consumption contrasted with the habits of Amsterdam's elite; Peter Burke, *Venice and Amsterdam: A Study of Seventeenth-Century Elites* (London, 1974), 84–93.

³⁷ G. Bistort, "Il Magistrato alle Pompe nella Repubblica di Venezia: Studio storico," *Miscellanea di storia veneta*, 3d ser., 5 (1912).

³⁸ Lane, *Venice: A Maritime Republic*, 270.

ioning the traditional image of Venice into a more contemporary one. In doing so, he employed some of the best artists of his time.

In 1527, over the opposition of the procurators, Gritti supported the appointment of Adrian Willaert as the *maestro di cappella* for San Marco, thus making Venice one of Europe's great musical centers.³⁹ After the Sack of Rome in that same year, Gritti welcomed to Venice the architect, Jacopo Sansovino, and the poet, Pietro Aretino, both of whom served the city for the rest of their lives. In his concern for improving churches, Gritti was the chief advocate of rebuilding San Francesco della Vigna and enriched the high altar in San Marco with tapestries ordered from Brussels.⁴⁰ But the most significant achievements of his reign were his efforts to bring order and harmony to the great ceremonial spaces of the city, the Piazza San Marco and the Piazzetta of the Ducal Palace, and to reorganize the state ceremonies themselves. During Gritti's reign, Sansovino made frequent attempts to remove the squalid wooden stalls around the columns at the entrance to the Piazzetta and to tear down the illegal shacks of the butchers and salami sellers who had infested both ritual spaces.⁴¹ In league with the procurators who had the actual jurisdiction over the area, Gritti sought to separate the commercial and political centers of Venice, freeing the government of nobles from the taint of commerce.

The centerpiece of the architectural reorganization of the republic's ceremonial space was the reconstruction of the "Loggetta." Standing opposite the Foscari arch at the intersection of the Piazza and the Piazzetta and complementing the great stairs where the doges were crowned, the Loggetta has been called "the most complete surviving visual representation of . . . the Venetian view of their own state as the perfect republic."⁴² In the last year of Gritti's dogeship the procurators commissioned Sansovino to rebuild the Loggetta to serve as a place for patricians to meet to discuss politics or conduct business with governmental officials and as an office for the procurators to use during the Sunday meetings of the Great Council.⁴³ Sansovino's designs show, however, a far greater interest on the part of the patrons in propagandistic display than in utility, for the Loggetta is quite small. The iconographic program used classical mythology and allegory to translate the traditional political imagery of Venice into Renaissance terminology. On the upper level three reliefs express the city's imperial claims: "Venice" symbolized by Justice sits in the middle flanked by personifications of the mainland rivers, representing the cities of the *terra firma* domain; on either side Venus as queen of Cyprus and Jupiter as king of Crete signify the pillars of Venice's maritime empire. Beneath these, four bronze statuettes of Pallas Minerva, Mercury, Apollo, and Peace represent respectively wisdom, eloquence, liberty

³⁹ E. Rosand, "Music and the Myth," 518-19.

⁴⁰ Howard, *Jacopo Sansovino*, 4, 66; and Boucher, "Jacopo Sansovino," 562-63.

⁴¹ Howard, *Jacopo Sansovino*, 10-16.

⁴² *Ibid.*, 34. Also see Pincus, *The Arco Foscari*.

⁴³ Pope-Hennessy, *Italian High Renaissance*, 404-05.

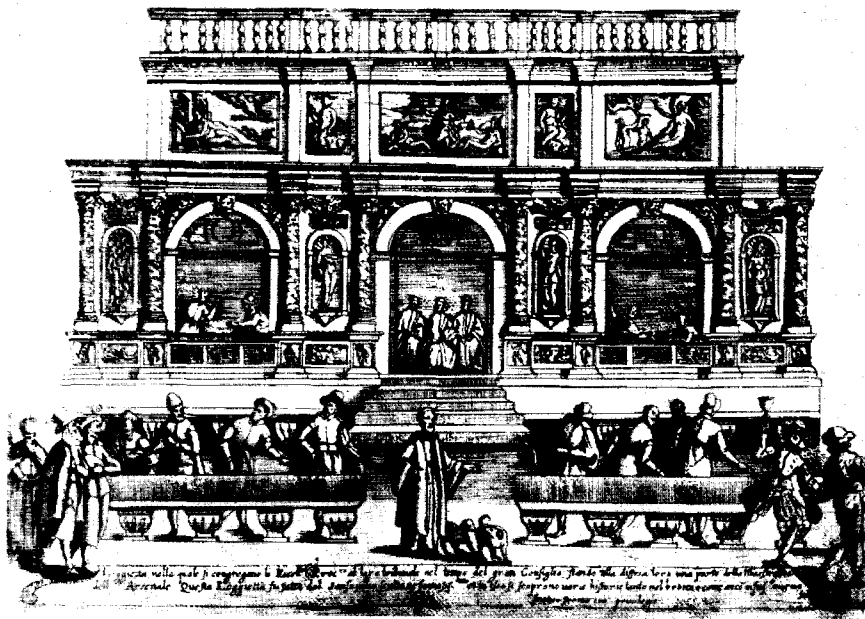


Figure 7: Giacomo Franco, "The Loggetta," detail of engraving, published in Venice in 1610.

under the law, and the state of Venice's relations with other cities.⁴⁴ With the completion of the Loggetta and Sansovino's Library and Mint across from the Ducal Palace, Venice gained a monumental center worthy of its claim to be a "New Rome" and a classical iconography to express its virtues.

Just as he was concerned with the music, decorations, and architecture of the governmental center, Doge Gritti was active in reforming the public ceremonies so they might reflect the full dignity of the state. Under the two previous doges, Leonardo Loredan and Antonio Grimani, the government eliminated several of the ritual vestiges of the greater powers the doges had wielded in the early centuries,⁴⁵ but Gritti was the more thorough reformer. His greatest change was the alteration of roles played by the senators and other magistrates at the festivities of Giovedì Grasso, the culminating holiday of Carnival. In the past, the doge and Signoria had witnessed the formal condemnation and decapitation of a bull and twelve pigs in the Piazzetta in memory of the defeat of a twelfth-century rebel patriarch of Aquileia and his Friulian allies. After the gruesome punishment of the symbolic captives, the doge and dignitaries retired to the Ducal Palace where they watched septuagenarian senators, brandishing clubs, smash miniature wooden models of the Friulian castles destroyed after the Venetian victory.⁴⁶ Thinking the ceremony

⁴⁴ Howard, *Jacopo Sansovino*, 29–35. For Francesco Sansovino's description and interpretation of the sculptural program designed by his father, see Pope-Hennessy, *Italian High Renaissance*, 404–05; and, in part, E. Rosand, "Music and the Myth," 513–14, n. 6.

⁴⁵ Martino da Canale, "La cronique des Venetiens," *Archivio storico italiano*, 1st ser., 8 (1845): 578–79, 745 n. 311.

⁴⁶ *Ibid.*, 576–79; Giustina Renier Michiel, *Le origine delle feste veneziane*, 2 (Milan, 1829): 37–45; Giuseppe

too undignified for the times, Gritti eliminated it altogether, retaining only a bull-baiting to entertain the crowds, and he and his successors called for more “noble” entertainments, such as comedies, ballets, masquerades, fireworks, and pageants.⁴⁷

WITHIN THE CONTEXT OF SHIFTS IN SOCIAL VALUES toward a more noble lifestyle, an oligarchical concern for authority, and the spread of classical motifs in the visual arts, Venetian pageantry blossomed, an exotic flower grafted onto a hardy ceremonial tradition. Always known for the great pomp of its rituals and processions, by the last half of the sixteenth century the very name of Venice conjured up a vision of extravagant spectacles.⁴⁸ In attempting to find a fitting description for the splendor of the lord mayor of London’s cortege in 1610, Christianus, the prince of Anhalt, invoked the reputation of Venice, declaring that no other state or city in the world accompanied the election of its magistrates with as much magnificence as Venice, which on this occasion the City of London nearly equaled.⁴⁹ And, as the Venetian nobles’ interest in commerce declined, their care for splendid pageantry grew.

To the historian the most obvious attribute of Venetian pageantry is its overwhelmingly political character. An art form created for a particular liturgical, diplomatic, or governmental occasion, pageantry revealed how its patrons wished an issue to be understood; through pageantry, for instance, an ideal vision of the political world might be used to mask the unseemly reality of a specific event. In sixteenth-century Italy and Europe, pageantry, though transitory, became a major art form that taxed the ingenuity of some of the greatest painters, sculptors, architects, composers, and poets of the Renaissance. In Florence Filippino Lippi and Pietro Perugino created floats and redecorated the Medici Palace for the entrance of Charles VIII of France in 1494; in Milan Leonardo designed his legendary *macchine* and pageants for

Tassini, *Feste, spettacoli, divertimenti, e piaceri degli antichi veneziani* (2d ed., Venice, 1961), 23–24; and Bianca Tamassina Mazzarotto, *Le feste veneziane: I giochi popolari, le cerimonie religiose e di governo* (Florence, 1961), 31–32.

⁴⁷ “Il quale uso, parendo al Principe Gritti, che fosse ridicolo affatto, se bene ordinato da gli antichi Padri, fu del tutto levato via”; Sansovino, *Venetia* (1663 ed.), 406. Also see Michiel, *Le origine delle feste veneziane*, 43. After the reforms of Doge Gritti, the Council of Ten took direct control of the Giovedì Grasso festival providing fifty ducats per year for the cost of the bulls, games, and decorations. Sanuto, *I diarii*, 44: col. 171. In 1549 (1550 new style) the Ten transferred supervision of the festival to the Rason Vecchie, limiting annual expenditure to one hundred ducats. Archivio di Stato, Venice [hereafter ASV], Rason Vecchie, busta 1, f. 114, and busta 3, f. 169v. Also see MCV, Cod. Cicogna 2991/I 25, f. 4r. Tassini incorrectly gives 1594 as the date of the transfer of control; *Feste, spettacoli*, p. 24, n. 1. The Rason Vecchie coordinated public and private expenditures, organized special entertainments, and selected the best plans for festival decorations and *macchine* from proposals submitted by artists who were often of high caliber. Unfortunately, there are no extant sixteenth-century proposals as there are for the seventeenth and eighteenth centuries. See ASV, Rason Vecchie, buste 225 and 226. Antonio Pellanda published one of the seventeenth-century proposals from busta 226 in *A Giacinto de’ Mitri nel giorno faustissimo del suo matrimonio colla gentile signora Erminia Balliana* (Venice, 1878), 19–21.

⁴⁸ For examples, see Francesco Petrarch, *Letters*, selected and translated by Morris Bishop (Bloomington, Ind., 1966), 234–39; and Pietro Casola, *Viaggio a Gerusalemme* (Milan, 1855), 109, translated into English by M. Margaret Newett as *Canon Pietro Casola’s Pilgrimage to Jerusalem in the Year 1494* (Manchester, 1907).

⁴⁹ Cited in Jean Robertson, “Rapports du poète et de l’artiste dans la preparation des cortèges du Lord Maire (Londres 1553–1640),” in Jacquot, *Les fêtes de la renaissance*, 270.

Lodovico Sforza, "Il Moro." In Venice, although relatively obscure artists were often employed to design public pageants, on occasion Titian, Carpaccio, Tintoretto, and Palladio employed their talents to create floats, *macchine*, allegorical scenes, and triumphal arches.⁵⁰ Willaert and Gabrieli composed music for some of these occasions, Aretino contributed verse, and leading humanists usually drew up the programs and planned their iconography.⁵¹ In great water processions, regattas of decorated barges were rowed up the Grand Canal or across the lagoon to celebrate the annual Marriage of the Sea on Ascension Day, the visits of foreign princes, or special events like the coronation of a dogaressa. All such pageants, however, began or ended with a procession on foot around Piazza San Marco and a visit to the basilica, as one has seen, the ceremonial and liturgical stage devoted to the presentation of the ideals of the Venetian republic. Pageantry was not an independent activity but one rooted in the great cycle of rituals conducted by the government. Indeed, both pageantry and the decorations of San Marco and the Ducal Palace illustrated the political ideas that underlay the ceremonies of state.⁵²

As was the case for all publicly commissioned works of art, the planning of pageantry was a matter of government policy. Patricians, appointed to special committees for important occasions, often invented the programs themselves or at least approved what someone else drew up, and magistrates at the highest levels of government including members of the Council of Ten, the Collegio of the Senate, and the procurators made direct policy decisions with regard to the occasions for and even to the detailed subject matter in pageantry devices.⁵³ For example, during the War of the League of Cambrai, the Council of Ten ordered that all tableaux made for a forthcoming ceremony be inspected by one of their secretaries before they could join the procession and that, despite the fact that enemies surrounded Venice, none of the tableaux should depict anything offensive to any king or potentate in the world.⁵⁴ In this instance the demands of careful diplomacy outweighed the opportunity for jingoistic propaganda, and a visual display of Venice's imperial ambitions

⁵⁰ Lina Padoan Urban, "Apparati scenografici nelle feste veneziane cinquecentesche," *Arte veneta*, 23 (1969): 143-55.

⁵¹ E. Rosand, "Music in the Myth"; Juergen Schulz, "Vasari at Venice," *Burlington Magazine*, 103 (1961): 500; and MCV, Cod. Cigogna 3278/24, f. 32r.

⁵² Sinding-Larsen, *Christ in the Council Hall*, xl. The same, of course, is true of the music performed in San Marco; see E. Rosand, "Music in the Myth." Michelangelo Muraro has argued that the influence of Venetian pageantry *macchine* can be seen in the architecture of the church of Santa Maria del Salute; Muraro, "Palladio et l'urbanisme vénétien," in Pierre Francastel, ed., *L'urbanisme de Paris et l'Europe, 1600-1690* (Paris, 1969), 216-17.

⁵³ Descriptions of all ceremonial occasions and copies of decrees made about them are contained in manuscripts in the Archivio di Stato, Collegio Cerimoniale series, which were begun in 1593 but include accounts for the entire sixteenth century. For earlier periods, see ASV, Regina Margherita, B-14, series I,XXVI, n. 6 (formerly catalogued as Ex Brera 277). On state patronage of the arts in general see Logan, *Culture and Society in Venice, 1470-1790*, 181-93. Control over all official commissions was tight; for example Sinding-Larsen has noted that there are no known instances of artists themselves inventing a program for the decoration of the Ducal Palace; *Christ in the Council Hall*, 8. Control was maintained not only through the planning of subject matter for commissions but through the strict regulation of the painters' guild by the office of the Giustizia Vecchia. D. Rosand, "The Crisis of the Venetian Renaissance Tradition," 27-29.

⁵⁴ Sanuto, *I diari*, 16: cols. 284-90.



Figure 8: Gentile Bellini, "Procession in Piazza San Marco of the Scuola Grande San Giovanni Evangelista" (1496). oil painting. Photograph courtesy of Accademia dei Belli Arti, Venice.

was avoided. A ducal chancellor regularly acted as master of ceremonies to handle organizational details, and the guilds, *scuole grandi* (lay confraternities), parishes, and monasteries that actually built the tableaux were required to participate in the procession under pain of a stiff fine.⁵⁵ Until 1565 the *compagnie delle calze*—groups of young nobles formed to sponsor banquets,

⁵⁵ The *maestro delle cerimonie* was under the supervision of one of the five *savi di terra firma* (officials of the executive council, the Collegio, of the Senate). For a description of the functions of the various officials in charge of ceremonies and the fines charged for nonparticipation, see BMV, Cod. Latin III, 172 (2276), ff. 63v–66r, 116r. Also see MCV, Cod. Cicogna 1295, p. 652; Sansovino, *Venetia* (1604 ed.), 314r; and Andrea da Mosto, *L'Archivio di Stato di Venezia: Indice generale, storico, descrittivo, ed analitico*, 1 (Rome, 1940): 22. The Basilica hired a *maestro di cappella* to take charge of the musical plans for the ceremonies; E. Rosand, "Music in the Myth," 521. On the *scuole grandi* in processions, see Pullan, *Rich and Poor in Renaissance Venice*, 33–62, 99–131. Although they were primarily charitable organizations, the *scuole* spent as much as one-third of their income on pageantry, pompous decorations, and buildings. Also see William Brooks Wurthmann, "The Scuole Grandi and Venetian Art, 1260–c. 1500" (Ph.D. dissertation, University of Chicago, 1975). For examples of the guilds' attempts to avoid the financial burdens that participation in pageants placed on them, see "Cronaca Agostini di Venezia," MCV, Cod. Cicogna 2853, p. 104; MCV, Mariogola IV, 102, ff. 94v–95v, 97v–98r; and Mario Tuto, *Ordine et modo tenuto nell'incoronazione della serenissima Moresina Grimani Dogaresa di Venetia: L'anno MDXCVII. adi 4. di maggio. Con le feste, e giochi fatti* (Venice, 1597), 4. For pageants and festival displays not destined to appear in Piazza San Marco, such as those that appeared in the working district of Santa Maria Formosa during Carnival, a local "intellectual"—a lawyer or priest—was put in charge. For an example, see Sanuto, *I diarii*, 19: col. 441.

comedies, and festivals for their own entertainment and for the glory of the republic—usually planned and even financed pageants as training for higher responsibilities.⁵⁶ To design and execute their contributions to public festivities, the companies appointed each year a poet, an architect, and a painter.⁵⁷ The ruling nobility not only took their own processions seriously but were deeply interested in accounts of foreign spectacles, seeing in them clues to possible shifts in policy and using them as a catalogue of the political ideas current in distant courts.⁵⁸ Although pageants often displayed governmental magnificence for its own sake and were reminiscent of the *circenses* with which imperial Romans entertained and controlled the crowd, they were more than that. Their planners used them to explicate specific and frequently quite sophisticated political ideas.

IN COMPARISON WITH THE OTHER ARTS, pageantry appeared late in the artistic development of Renaissance Venice. In the context of this study, pageantry refers to all of the artistic elements created for a particular ceremonial occasion, including triumphal arches, pageant stages, display booths, decorated floats and barges rowed on the lagoon and canals, and tableaux vivants set up on platforms and carried in procession around Piazza San Marco. The portable tableaux became the characteristic device for pageantry in Venice, seen far more frequently than floating *macchine* or fixed stages. Before the sixteenth century, tableaux employing either live actors or groups of wooden or plaster statues were rare. Proto-tableaux were at first appendages to the ducal processions and for centuries displayed only the insignia of the doge or a relic, miraculous painting, or statue of a saint.⁵⁹

⁵⁶ Sansovino, *Venetia* (1604 ed.), 273v–74r; Bernardo Giustinian, *Historie cronologiche dell'origine dell'ordine militari e di tutte le religioni cavalleresche infino ad hora instituite nel mondo . . .* (Venice, 1692), 111–15; Lionello Venturi, "Le compagnie della calza (sec. XV–XVI)," *Nuovo archivio veneto*, new ser., 16 (1908): 161–221 and 17 (1909): 120–233; *Narrazione della festa solenne data in Venezia dalla Compagnia della Calza nel MDXX adì XIII Febraro per l'accettazione di tre socii* (Venice, 1852); Michelangelo Muraro, "Vittore Carpaccio o il teatro in pittura," in Maria Teresa Muraro, ed., *Studi sul teatro veneto fra rinascimento ed età barocca* (Florence, 1971), 7–19. Even after the *Compagnie* were abolished in 1565, when the occasion called for a spectacular pageant similar groups would be formed on an *ad hoc* basis; Tutio, *Ordine*, 5, 16.

⁵⁷ Giustinian, *Historie cronologiche dell'origine degli ordine militari*, 115.

⁵⁸ Sanuto, *I diarii*, 8: col. 545, 29: cols. 103–06.

⁵⁹ Regarding the doge's insignia, "Andando adunque in trionfo, & con solennità, porta con lui fra l'altre, sette cose degne di consideratione, & dimerstratrici della sua molta eccellenza. Le quali egli hebbe da i primi Principi del mondo, cioè da i Pontefici, & da gli Imperatori"; Sansovino, *Venetia* (1604 ed.), 321r–v. Also see *ibid.*, 315v–322v. For the close identification of these insignia with the doge, see ASV, Collegio Cerimoniale, I, f. 9v; BMV, MS Latin III, 172 (2276), f. 54r; and MCV, MS Venier P.D. 517b, under heading "avertimenti generali." Also see Donato Giannotti, *Libro de la Republica de Vinitiani* (Rome, 1542), 65r–v; Hans Conrad Peyer, *Stadt und Stadtpatron im mittelalterlichen Italien* (Zurich, 1955), 63; Agostino Pertusi, "Quedam regalia insignia: Ricerche sulle insegne del potere ducale a Venezia durante il Medioevo," *Studi veneziani*, 7 (1965): 82–91; Lina Padoan Urban, "La festa della Sensa nelle arte e nell'iconografia," *ibid.*, 10 (1968): 291–311; and Gina Fasoli, "Liturgia e cerimoniale ducale," in Agostino Pertusi, ed., *Venezia e il Levante fino al secolo XVI*, 1 (Florence, 1973): 261–95.

To emphasize the untouchable holiness of sacred objects and to compete for attention with the rich white, black, scarlet, and vermillion robes of the noble participants, the canons and *scuole grandi* members carried ever larger, more gilded and bejewelled reliquaries, which eventually required four or more men to support them. People in the procession began to use portable platforms (*solari*) on which they carried their reliquaries and statues of saints, sometimes made for the occasion. When the little dramas that could be told with groups of statues or actors took over as the primary visual elements in the processions, tableaux had reached their maturity. Even though there are numerous descriptions of brilliant processions in the fifteenth century, the visual domination of tableaux over the other elements of the procession does not seem to have been common before the first decade of the sixteenth century.⁶⁰ This is not to say, however, that the processions held before the development of tableaux were without political impact. Besides the elaborate rankings of noble officials and the imperial bearing of the doge, the very orderliness and solemnity of the processions impressed foreigners as unparalleled, and processions were often read as a commentary on the harmony of Venetian society.

At the end of the fifteenth century, for instance, a Milanese pilgrim, Canon Pietro Casolo, described the Corpus Christi procession in which he participated on the eve of his embarkation for Jerusalem: "A great silence was maintained, more than I have ever observed on similar occasions, even in seating so many Venetian gentlemen; every sound could be heard. One single person appeared to me to direct everything, and he was obeyed by everyone without protest. This filled me with astonishment, because I had never seen such perfect obedience at similar spectacles elsewhere."⁶¹ For Casolo, at least, a state procession—held, ostensibly, for a purely religious occasion—mirrored the Venetian reputation for political order.

The earliest tableaux in Venice took their peculiar shapes from the long-standing Venetian tradition of turning art and religious themes to political

⁶⁰ On the festival of the Marys that included a procession of statues and actors but was abolished in the fourteenth century, see Silvio Tramontin, "Una pagina di folklore religioso veneziano antico: La festa de 'Le Marie,'" in *La religiosità popolare nella valle padana*, Atti del II convegno di studi sul folklore padano, Modena, 19–21 Marzo 1965 (Modena, 1966), 401–17. In the particularly sumptuous Corpus Christi procession of 1440, which also celebrated a Venetian victory, there is no mention of tableaux of any kind; MCV, Cod. Cicogna 2043, ff. 25–27. The same is true for Corpus Christi in 1468 and 1495; MCV, Cod. Cicogna 2991/11, ff. 13r–24r. The earliest record of tableaux at Corpus Christi of which I am aware is for the procession of 1506: "fu fato una bellissima precessione, le scuole a rogata si feno honor, con molte demonstration et soleri"; Sanuto, *I diarii*, 6: col. 350. By 1510 pageant floats were considered normal as Sanuto revealed when he commented about a procession that lacked them: "la procession ferial, non soleri ni anzoli chome li anni passati"; *ibid.*, 10: col. 460. Also see *ibid.*, 12: col. 243. One can trace the growing sophistication and complexity of the tableaux in *ibid.*, 14: col. 306; 16: cols. 303–04; 18: col. 271; 20: cols. 274–75; 24: cols. 347–48; 25: col. 437; 27: col. 405; 30: col. 281; 34: col. 238; 36: col. 369; 39: col. 77; 41: col. 414; 45: col. 356; 50: col. 374; 58: col. 315. Also see BMV, MS Latin III. 172 (2276). ff. 16v–17r; and Doglioni, *Le cose notabili*, 72.

⁶¹ "Uno grande silentio se tene, e più che mai vedeti tenere a simili spectaculi, etiam in lo assetar tanti zentilhomini veneziani, ita che ogni cosa potete intendere. E uno solo a me pariva governasse ogni cosa, el qual senza resistentia era da ogni homo obedito. E da questo pigliai grande admiratione, perchè non vidi mai tanta obedientia a tali spectaculi." Casolo, *Viaggio a Gerusalemme*, 16, as translated in Newett, *Canon Pietro Casola's Pilgrimage*, 147.

ends. At a procession formed in 1511 during the War of the League of Cambrai, religious motifs were used to defend Venice's interests in the latest diplomatic alignment of powers with and against the republic. The members of the Scuola Grande di San Rocco built a tableau with statues depicting Justice, Saint Roch, Saint Mark, and a woman dressed as Venice holding a dove representing the Holy Spirit; they were accompanied by two kings on horseback, the kings of Spain and England, who had recently joined the Venetian cause, and by a ship with a sign reading "nolite timere, cessavit ventus." Venice's remaining enemy, the king of France, faced a flaming ball, representing *Amor Dei*, and beside him stood the pope with a placard questioning why France had denied the true faith.⁶² To oppose Venice was clearly to become God's enemy. Religious images continued to dominate Venetian pageantry tableaux for several decades. The signing of a new league in 1526 was celebrated with a procession of tableaux that compared the doge to Joshua and represented each of the alliance's members as under the guidance of the divine.⁶³ Even as late as 1532, pageants still borrowed imagery exclusively from the Christian tradition and concentrated as much on the display of precious objects as on creating dramatic scenes on *solari*. At one procession that year the Scuola Grande di San Rocco, by then famous for its tableaux vivants, built three *solari* to carry some of its members acting out scenes from the Old Testament, but it also brought to the procession a *solaro* adorned solely with silver liturgical objects and had sixty-six members walk in the procession carrying silverplate in their hands.⁶⁴ The show of material riches was still dominant, and less valued was ingenuity in creating allegorical scenes. While the image of Venice as the "New Rome" and classical allusions in the other arts had been gaining favor in Venice for some time, pageantry and tableaux were still closely tied to the liturgical elements of the procession. Jupiter and Venus had not yet joined Mark and Mary on the ceremonial stage.

Perhaps fostered by the Venetians' anxious quest for *nobiltà* and Gritti's campaign for monuments and ceremonies more suited to an empire, a major shift in the iconography of pageantry came in 1542. In March 1541 a group of twelve young nobles organized a new *compagnia delle calze* called the *Sempiterni*. From the very first they commissioned artists of the premier rank to create the company's festive decorations; for the first initiation party in Campo Santo Stefano, Titian designed a grand *apparato* or stage.⁶⁵ For Carnival in the following February the *Sempiterni* hired Pietro Aretino to write a comedy for them to produce. To design a floating stage on which to perform the play, Aretino sent for his young compatriot from Arezzo, Giorgio Vasari, who built a barge complete with architectural elements, sculptures, and paintings that represented the rivers, lakes, and islands of the Venetian domain.⁶⁶ Before

⁶² Sanuto, *I diarii*, 13: cols. 132-41.

⁶³ *Ibid.*, 42: cols. 62-78.

⁶⁴ *Ibid.*, 56: col. 286.

⁶⁵ Schulz, "Vasari at Venice," 500.

⁶⁶ MCV. Cod. Cicogna 3278/24, f. 32r.

this, the *compagnie delle calze* comedies had always been performed on a modest stage set up in a palace courtyard that had been decorated with tapestries, streamers, and silverplate to create a festive mood.⁶⁷ Juergen Schulz has pointed to Vasari's Carnival *apparato* as the source for the adoption in Venice of the elaborate stylistic motifs popular in central Italy, especially in the dramatic productions of the Medici court.⁶⁸ Although the Venetians did not entirely abandon their own largely religious iconography, Venetians of the 1540s and after increasingly accepted pageantry in which neoclassical motifs were properly transformed to serve Venetian ends. The mythical gods of antiquity joined the saints as symbols of the state, decoration that was merely sumptuous yielded to ingenious design, straightforward messages became complicated allegories, and carefully planned propaganda supplanted simple allusions to the divine guidance of the rulership.

Concurrently, the annual number of state processions that provided the opportunity for pageantry increased markedly in the late sixteenth century, and to this increase one music historian has credited the extensive use in Venice of polychoral music, a form well suited for outdoor ceremonies.⁶⁹ Significantly, the stylistic and iconographical transformation reveals more than a change in taste. The young nobles who had joined the *compagnie delle calze* during the 1540s and sponsored the introduction of the new themes eventually became senators. By the 1570s they had entered the councils of power, and the government officially endorsed the use of the classical style for state pageantry. By this time the allusions to antiquity seldom served the ends of an ambitious doge, as had the staircase in the courtyard of the Ducal Palace for Agostino Barbarigo, but polished the noble image of the patriciate and proclaimed the authority of the government. The new pageantry revealed a political ethic that emphasized obedience rather than communal responsibility, and, despite the apparent resurgence of opposition to the oligarchy in the 1580s and the appearance of discourses on republican government by patri-

⁶⁷ Sanuto, *I diarii*, 19: cols. 443, 439; 27: col. 653; 29: col. 547; 52: col. 513; 54: col. 35; and Schulz, "Vasari at Venice," 505–06.

⁶⁸ Schulz, "Vasari at Venice," 505–06. By 1565, when Palladio built a wooden *teatro* for a *compagnia*, the style was firmly implanted in Venetian pageantry; MCV, Cod. Cicogna 3278/24, f. 2r.

⁶⁹ On music, see E. Rosand, "Music and the Myth," 534. Rosand is correct in noting the increase in ceremonies during the sixteenth century; *ibid.*, 516. The pattern is, however, more complex than she has indicated. In the thirteenth century Martino da Canale found twelve annual festivals worthy of discussion; "La cronique," 558–605. By the end of the fifteenth century Marin Sanuto recognized twenty important days; "Le vite dei dogi," in Giosue Carducci, ed., *Rerum italicarum scriptores*, Raccolta degli storici italiani, 22 (2d ed., Bologna, 1900): pt. 4, nos. 1–5, pp. 86–91. The official ceremonial book of the state lists sixteen annual events that required the participation of the doge and Signoria during the sixteenth century and also shows that the number of events varied greatly from year to year; ASV, Collegio Cerimoniale, 1: f. 11r–v. Other lists, which included many of the locally celebrated saints' days, named twenty-nine, sixty-nine, and seventy-eight annual days of ceremonial importance. See, respectively, BMV, MS Latin III, 172 (2276), f. 54r–v; MCV, Cod. Cicogna 2991/128, ff. 1r–5v; and MCV, MS Donà delle Rose, 132/5, ff. 139r–143v. Comparing a number of sources, I have found at least eighty-six different days that by the end of the sixteenth century had some ceremonial importance for the republic. Sansovino distinguished between the ten holidays requiring a full procession comprised of the doge, Signoria, and the display of the ducal symbols and the four holidays conducted without the symbols and musicians; *Venetia* (1604 ed.), 330v, 243v–45v. Seven of these fourteen days also required the participation of the *scuole grandi*, clerical congregations, and orders of regulars, and it was on these occasions that tableaux vivants and musical performances were likely to be appropriate; BMV, MS Latin III, 172 (2276), f. 55v.

cians such as Paolo Paruta, the public image of *La Serenissima* grew ever less republican, ever more authoritarian.⁷⁰

THE LAST THREE DECADES OF THE SIXTEENTH CENTURY became the great age of Venetian pageantry. The magnificence of the antique style appealed to the ruling elite, who were concerned with projecting an image of strength and majesty both at home and abroad in the face of renewed external threats to Venice's imperial reputation, beginning with the loss of Cyprus to the Turks in 1573. This calamity was followed by a devastating plague in 1575–77 that killed over one-third of the population and by the fires of 1574 and 1577 that destroyed the council chambers of the Ducal Palace. All of these events brought into question the conception of Venice as a city favored by God. The patricians in charge of planning responded by developing two artistic themes. In one, Venice's steadfast Catholic orthodoxy was contrasted to the machinations of the insidious heretics and powerful infidels who menaced Europe, and, in the other, Venice's situation was compared to Imperial Rome's struggle to rally forces against the barbarians. The themes were pointedly self-serving: without the Venetians would not all Europe fall to the sword of the Turk or to the perversions of the Protestants?

The 1570s had begun well enough. In October of 1571 the combined navies of the pope, Spain, Venice, and some minor Catholic powers defeated the Turkish fleet near Lepanto in the Gulf of Patras in Greece. At the joyous victory celebration in Venice, the traditions of depicting Venice in terms of religious virtues and through various personifications of the classical gods were combined with satirical portraits of the humiliated Turks. A tableau of one *scuola grande* compared Pius V, Philip II, and Doge Alvise Mocenigo (represented by Neptune) to the cardinal virtues—Faith, Hope, and Charity; on the same *solaro* three youths stabbed a huge dragon with a crescent on its head, symbolizing the Grand Turk. On the other *solari* rode figures of Faith, Rome, Spain, the doge, and Victory as if in triumphal chariots, another classical allusion. At a celebration held later at the Rialto, an ingenious illuminated pyramid turned on its axis, bearing statues and paintings of Neptune, Jupiter, Saturn, and Mars.⁷¹

Despite the loss of Cyprus, Venice's richest Eastern possession, in the peace negotiations with the Turks in 1573, the exultant tone of pageantry persisted. When Henry III of France visited Venice in 1574 on his way to be crowned Most Christian King after spending an unhappy year as the king of Poland,

⁷⁰ Opposition was manifest in attempts to reform the Council of Ten in 1582. Cozzi, *Il Doge Nicolò Contarini*, 206; and Bouwsma, *Venice and the Defense of Republican Liberty*, 226–28, 242–49, 250–54. For criticisms of their interpretation of events of 1582, see John Martin Clement Lowry, "The Reform of the Council of Ten, 1582–3: An Unsettled Problem?" *Studi veneziani*, 13 (1971): 275–310; and Brown, "Nicolò da Ponte," 145–58.

⁷¹ E. H. Gombrich, "Celebrations in Venice of the Holy League and of the Victory of Lepanto," in *Studies in Renaissance and Baroque Art Presented to Anthony Blunt on His 60th Birthday* (London, n.d.), 62–68. In addition to the sources used by Gombrich, see Nicolò Trevisan, "Cronaca veneta," BMV, MS italiano VII, 519 (8438), ff. 330v–331r; ASV, Collegio Cerimoniale, 1: ff. 38v–39r; and Francesco da Molino, "Compendio . . . della cose . . . che succederanno in mio tempo sì della Republica Venetiana, e' di Venetia mia patria . . .," BMV, MS italiano VII, 553 (8812), ff. 29–32.



Figure 9: Andrea Michieli, called il Vicentino, "The Ingress of Henry III." engraving. Photograph courtesy of Museo Civico Correr, Venice.

the entertainments in his honor were the most expensive and spectacular of the Venetian Renaissance. From the triumphal arch to the sugar sculptures served as dessert at the state banquets, the decorations had a consistent theme of courting French favor at the expense of the Spanish.⁷² A committee of nobles, including two already well known for their humanistic interests and their patronage of Palladio, planned the decorations, coordinating the diplomatic intentions of the state with the proper Neoclassical allusions. The major creation for the visit was a wooden triumphal arch built on the Lido by Palladio and decorated by Veronese and Tintoretto in imitation of the arch of Septimius Severus in Rome; on it the escutcheons of Venice and France were paired under statues of Victory and Peace, and around the arch were paintings of battles that Henry had won against the Huguenots.⁷³ Classical images served an overall scheme that had strong military and religious tones: Venice was the natural partner of France because the city was the bulwark against the heathen Turk, just as the French kings were the stalwart protectors of the Catholic cause against the heretics.

The late sixteenth century's greater enthusiasm and appreciation for the dramatic as opposed to the merely spectacular was also marked in pageantry. The visit in 1585 of some Japanese nobles, newly converted to Christianity by Jesuit missionaries, offered a consummate opportunity for didactic drama. The Collegio postponed the procession scheduled for Saint Mark's Day (June 25) to coincide with the festival of Saints Peter and Paul three days later and ordered that the usual midsummer frolics and games be suspended so the city could prepare a properly spiritual and devout holiday for the guests.⁷⁴ Built to tell "as in a theater" the sacred history of the Old and New Testaments and the trials of the saints and martyrs, three hundred *solari* were accompanied by an unprecedented display of reliquaries, jewels, and silver liturgical objects. The six *scuole grandi*, the Dominicans and Franciscans, and other religious orders created tableaux vivants instructing the Japanese about Christian truths and, inevitably, about Venice's special place in the divine plan. There were the usual personifications of Venice as a queen surrounded by the virtues and legions of saints, but there were also attempts to explain complex subjects, such as the local legend of Saint Mark's gift of his episcopal ring to a Venetian fisherman, Solomon's demonstration of his wisdom and wealth to the queen of Sheba, and the baptism of Constantine and his subsequent charity to the poor.⁷⁵ Likewise, at the festivities following the league signed between Philip II and Henry IV in 1598, which ended years of hostilities between the two major Catholic powers, intricate presentations of religious and classical subjects dominated the tableaux. The Scuola Grande di San Rocco contributed a series of tableaux allegorizing the continents, including young girls

⁷² Nicolas Ivanoff, "Henri III à Venise," *Gazette des Beaux-Arts*, 80 (1972): 313-30.

⁷³ Logan, *Culture and Society in Venice, 1470-1790*, 186; and Pier de Nohac and Angelo Solerti, *Il viaggio in Italia di Enrico III re di Francia e le feste a Venezia, Ferrara, Mantova, e Torino* (Turin, 1890), 98-99.

⁷⁴ Guido Gualtieri, *Relazioni della venuta de gli ambasciatori Giaponesi a Roma, fino alla partita di Lisbona . . .* (Venice, 1586), 118-19.

⁷⁵ On the comparison of the Ducal Palace with Solomon's Palace, see note 16, above.



Figure 10: Andrea Michieli, called il Vicentino, "The Coronation of Dogressa Morosina Grimani," painting. Photograph courtesy of Museo Civico Correr, Venice.

riding on a bull, camel, crocodile, and rhinoceros representing respectively Europe, Asia, Africa, and America.⁷⁶ In another series, the Scuola Grande di San Giovanni Evangelista created a timely exposition on the evils of war: one witness could not find the words to describe the “thousands of ingenious effects imitating the confusion war caused on earth.” Neptune lamented anarchy on the seas; Death triumphed over corpses; castles burned; a company of soldiers abducted a young girl; and four “infernal furies” tied up a large globe.⁷⁷ Pageantry had become political pedagogy of a high order.

Two final examples serve to illustrate the effectiveness of pageantry as a political tool. In one case late in the sixteenth century, it was used to leap republican barriers; in another it helped restore credibility in a time of diplomatic crisis. In 1597 Marino Grimani, elected doge two years before and wildly popular among the common people, announced his plans to revive the lapsed practice of having the doge’s wife “crowned” in a formal entrance ceremony to the Ducal Palace. The Collegio of the Senate accordingly appointed a master of ceremonies to oversee the preparations, organized an *ad hoc* committee of young nobles to provide and pay for entertainments, and assigned to the guilds responsibility for building display booths for the Ducal Palace.⁷⁸ The “coronation” of the dogaressa was almost a contradiction in terms, because she had never actually been crowned but was instead presented with a list of “promises” she swore to keep, in a rite that emphasized the formal restrictions on the behavior of the doge’s family.⁷⁹ The Grimani and Morosini families, however, transformed the occasion of this solemn republican pledge into a ceremony of vaunting ambition. The company of young nobles hired Vincenzo Scamozzi, a student of Palladio, to design a grand barge (*gran macchina*) in which Dogaressa Morosina Morosini Grimani was rowed in a procession on the Grand Canal, accompanied by a large retinue of Venetian noble women and her two dwarfs. Decorations on the barge’s columned loggia included a large Neptune riding the tail of a whale, a globe, and a scene in which Saint Mark himself crowned the kneeling doge and dogaressa.⁸⁰ Here again the definition of ducal powers was a problem, for in this scene Saint Mark’s coronation of the couple ignored the reality that elec-

⁷⁶ The source is probably incorrect here since the crocodile was usually an attribute of America and the rhinoceros of Africa.

⁷⁷ Da Molino, “Compendio . . .,” ff. 176–82.

⁷⁸ Tutio, *Ordine*, 5, 16; Giovanni Rota, *Lettera nella quale si describe l’ingresso nel palazzo ducale della serenissima Morosina Morosini Grimani Principessa di Vinetia . . .* [hereafter *L’ingresso*] (Venice, 1597), 8; F. M. Piave, “Feste fatte in Venezia pella incoronazione della Serenissima Dogaressa Morosina Morosini Grimani,” *Emporio artistico letterario*, 11: 11; Sansovino, *Venetia* (1604 ed.), 280r; Giovanni Carlo Sivos, “Vite de Dosi di Venezia,” *Libro Terzo* (1595–1615), BMV, MS italiano VII. 1818 (9436), f. 9r-v; Doglioni, *Le cose notabili*, 107–33; and Pompeo G. Molmenti, *La dogaressa di Venezia* (2d ed., Turin, 1887), 305–26.

⁷⁹ Girolamo Oriani, ed., *Il trionfo della dogaressa di Venezia nel secolo XVI* (Venice, 1873); Gregorio Marcello, *Ordine et progresso [sic] del trionfo fatto l’anno MCLVII. all’ 19. di settembre. per l’incoronatione della Serenissima Dogaressa Priola* (Venice, 1597); and *Il trionfo et le feste fatte in Venetia nella publica entrata della Serenissima Dogaressa, moglie dell’Illustrissimo Signor Lorenzo di Priuli, Principe di Venetia* (Venice, n.d.).

⁸⁰ Rota, *L’ingresso*, 16–18; and Tutio, *Ordine*, 5–6. Also see Lina Padoan Urban, “Teatri e ‘Teatri del Mondo’ nella Venezia del Cinquecento,” *Arte veneta*, 20 (1966): 143–44. The barge depicted in figure 10 differs from this description in several details; I have relied primarily upon the literary evidence, since it is dated closer to the event.

tors had chosen Grimani for his office. Saint Mark was not just an aide to the doge, as in the revised version of the Veronese votive portrait of Doge Sebastiano Venier, nor was the doge representing the community in humble supplication to the saint; it was as if Saint Mark had personally chosen Marino and Morosina Grimani for “global” domination.

As she disembarked at the Piazzetta, Morosina Grimani walked through a triumphal arch that exalted the nobility of the two families.⁸¹ Surmounting the arch was a statue of a woman (Venice) holding a staff (authority) and bundles of wheat (prosperity), surrounded by paintings of the Venetian dominions.⁸² Symbols of the offices held by the Grimani and Morosini ancestors—royal and ducal crowns, cardinals’ hats, episcopal staffs, a patriarchal cross, a legate’s caduceus, and generals’ batons—complemented the arms of the two families. There were specific references to Doge Grimani’s eloquence as a legate to the Holy See, to his charity as a procurator, to his justice as a provincial governor, and to his popularity.⁸³ Repetitions and variations of the same theme continued as the dogaressa proceeded past the guilds’ booths to her “arrival” in the private chambers of the Ducal Palace.⁸⁴ Relying largely on pageantry, the Grimani turned upside down the tradition of the dogaressa’s coronation, transforming the restrictive ritual, during which she swore to abide by the lengthy legal proscriptions on her conduct, into a court festival that taunted the republican traditions of patrician equality with the aristocratic pretensions of the oligarchy. Grimani could not at his own coronation legally claim to be as powerful as he hoped to be, but carefully directed pageantry made his ambition clear at the “coronation” of his wife.

The last illustration of particularly effective pageantry is drawn from the most famous event in the history of the “political reputation” of Venice. The very decline of the republican ideals in practice may have corresponded with a reassertion of them in theory, and it was this theoretical defense of republican values more than the realities of Venetian political life that garnered for *La Serenissima* its European reputation as the ideal living republic. Venice’s reputation for the “defense of republican liberty,” as William J. Bouwsma has called it, was immensely strengthened, particularly in Protestant countries, by its adamant resistance to papal interference in Venetian domestic affairs. The conflict between Venice and the papacy reached its highest pitch when Pope Paul V put Venice under interdict from 1606 to 1607, and Venice’s most famous and long-lasting defense dates from this event, eloquently expressed in the writings of Paolo Sarpi, a Servite friar, who argued passionately against the supposed desire of the pope to establish a universal monarchy. The government, for its part, wanted to show the world that it had the complete

⁸¹ The butchers’ guild (*macellari*) built the arch according to the design of a miniaturist named Bernardo Fogari, who had consulted with a humanist lawyer, Attilio Facio; Piave, “Feste,” 50.

⁸² The paintings moved Rota to a long discourse on the glories of the Republic; *L’ingresso*, 25.

⁸³ *Ibid.*, 21–31; and Tutio, *Ordine*, 11–13. Also see Lina Padoan Urban, “Apparati scenografici nelle feste veneziane cinquecentesche,” *Arte veneta*, 23 (1969): 152–55.

⁸⁴ Rota, *L’ingresso*, 37–51; and Tutio, *Ordine*, 6–10. At the coronation of Zilia Dandolo Priuli in 1556 there was only one booth with an allegorical display. See Oriani, *Il trionfo*.

loyalty of its subjects even on religious issues by carrying on the celebration of the liturgy in defiance of the interdict.⁸⁵

Thus, the Corpus Christi procession of 1606 provided the opportunity for a demonstration of popular resistance to the interdict and a display of antipapal propaganda through pageantry, a manifestation so wonderfully successful that the sympathetic English ambassador, Sir Henry Wotton, called it "the most sumptuous procession that ever had been seen here," and the Jesuit spy, Giacomo Lambertengo, derided it as a "spettacolo miserabile."⁸⁶ The congregations of secular priests, most of the orders of the regulars, and the *scuole grandi* participated in the procession—the latter contributing numerous tableaux that, according to the official euphemism of the day, had "some scenes which alluded to the reasonable claims of the republic against the pope."⁸⁷ In particular, the tableaux proclaimed the distinctions between sacred and secular authority on which the Venetians built their case. On one *solaro* an actor dressed as Christ stood above a Latin motto quoting Mark 12:17: "Render unto Caesar that which is Caesar's and unto God what is God's"; and, on another, Christ reminded his Apostles that their priesthood did not allow them to usurp the authority of kings who properly ruled over the temporal affairs of mankind. The most pointed reference to the follies of the pope was a collapsing church ("chiesa cadente") supported by the Venetian doge assisted by Saint Dominic and Saint Francis. On either side of the church, other friars held up broad swords, emblazoned with the motto, "VIVA IL DOSE."

The procession was a diplomatic coup. Some nine days earlier at Pentecost, when the Signoria had walked in the procession unaccompanied by any foreign ambassadors, rumors spread that Venice had been abandoned by its friends. To the delight of the crowds, both the French and the Imperial ambassadors made a belated appearance on Corpus Christi Day.⁸⁸ Ambassador Wotton saw exactly what was going on: "The reasons of this extraordinary solemnity were two as I conceive it. First, to contain the people in good order with superstition, the foolish band of obedience. Secondly, to let the pope know (who wanteth not intelligencers) that notwithstanding his interdict, they had friars enough and other clergymen to furnish out the day."⁸⁹ Apparently, the procession achieved these ends and helped to turn the diplomatic tide in Venice's favor.

In the course of the sixteenth century, pageantry had thus evolved into a signal art form and a public mirror meant to reflect images of political power.

⁸⁵ Bouwsma, *Venice and the Defense of Republican Liberty*, 417–82, 339–416; Pullan, *Rich and Poor in Renaissance Venice*, 55–62; and Enrico Cornet, "Paolo V e la Repubblica Veneta nuova serie di documenti (MDCV–MDCVII) tratti dalle deliberazioni segrete (Roma) del Consiglio dei Dieci," *Archivio veneto*, 6 (1873): 60–61.

⁸⁶ Logan Pearsall Smith, *The Life and Letters of Sir Henry Wotton*, 1 (Oxford, 1907): 350; and "Relazione dell'interdetto di Paolo V." ASV, Consultori in Jure, F. 537, f. 23v, as printed in Gaetano Cozzi, "Paolo Sarpi tra il cattolico Philippe Canaye de Fresnes e il calvinista Isaac Casaubon," *Bollettino dell'Istituto di Storia della Società e dello Stato Veneziano*, 1 (1959): 105.

⁸⁷ "Alcune rappresentazioni che alludevano alla pretenzione ragionevole della Repubblica con il Papa"; "Relazione dell'interdetto di Paolo V." 105.

⁸⁸ "Relazione dell'interdetto di Paolo V." 105–06.

⁸⁹ Smith, *Life and Letters of Sir Henry Wotton*, 350.

It had not superseded the other arts as a political device, but its pliable and adaptive form appealed to those trained to rule others. In pageantry the elite of Venice had a superior means of exploiting Machiavelli's dictum that to most men appearances mattered more than realities.

IN ALL OF THE EXAMPLES DISCUSSED, the arts disclosed political ideas by making analogies. Through symbolism and allegory the arts elevated a political idea—however self-serving, prevaricating, or mean—to a transcendent plane: doges resembled saints, the gods directed the fortune of war or diplomacy, and Venice itself was the epitome of the theological, political, and classical virtues. Although the images changed and pagan deities joined Christian saints in the city's pantheon, the analogical process remained the same. This form of reasoning was not unusual, for much of Renaissance political thought, even when crafted by the finest minds, depended on metaphors—the “King's Two Bodies,” the “Ship of State,” or the “Marriage of the Sea,” for example—to unveil in human terms the implications of a given principle or abstraction.⁹⁰ It is certain that many people took these comparisons seriously, but it is much more difficult to tell to what degree such a habit of thought influenced ordinary political perceptions and decisions. In any historical period and for any one person, it is probably impossible to determine the exact balance of ideology, belief, and objectivity in motivating a particular action. In Venice, however, it seems that ideology always played a considerable role among supporters and opponents of the regime; in the major Renaissance protests against the dictatorial habits of the oligarchs, opposition was always phrased as “restoration” of a traditional balance of responsibilities and privileges to all patricians, and the most tired clichés of the established order were used by the protesters to argue against that very order.⁹¹

Perhaps the most important attribute of political imagery in the arts was its persuasive power. Visual images cajoled belief by simplifying and distorting political issues, by ignoring objectionable facts, and by juxtaposing symbols that connected ideas that may not have had any relationship in logic or reality. Herbert M. Atherton has found a similar persuasive tendency in eighteenth-century political prints, and other instances of politics so translated into art can surely be traced to the present day.⁹² But for Renaissance Venetians, art was not just a gloss on public issues nor a mere reinforcement of status discriminations and hierarchical rank; rather, the arts provided a commentary upon the whole political and social order and specifically upon the nature of class distinctions, noble privilege, and sacred inherited institutions. If the art in Venice can be reduced to anything so simple as a “func-

⁹⁰ For examples see Kantorowicz, *The King's Two Bodies*; and Michael Walzer, *The Revolution of the Saints: A Study in the Origins of Radical Politics* (New York, 1974), 171–82.

⁹¹ Cozzi, “Domenico Morosini,” and *Il Doge Nicolò Contarini*, 229–83; and Rose, “Marc Antonio Venier.” Also see note 70, above.

⁹² Atherton, *Political Prints in the Age of Hogarth: A Study of the Ideographic Representation of Politics* (Oxford, 1974), esp. 65–66.

tion," then its function was interpretive: it was a Venetian reading of Venetian experience, a story they told themselves about themselves.⁹³

The relationship between patronage of the arts and political power is a less elusive problem: the richest men in the highest ranks of power preserved exclusive control over all political ideas in the arts. Only nuances of disagreement about political images in the arts, such as how to depict the powers of the doge, ever emanated from within this tight circle of men who rotated the important offices among themselves. The poorer nobles, the few privileged *cittadini*, and the disenfranchised masses were mute. Art may have once been the common tongue of the people, as Berenson believed, but a small group of powerful men chose what was to be said, and in the sixteenth century they increasingly chose a language that only an educated few could fully understand: what did fishmongers and gondoliers know of Jupiter, Mars, and Latin epigrams?

This elitism is not surprising. Anthropologists have shown that in many traditional societies village strongmen dominate over the most popular and universally accepted phenomena.⁹⁴ For Western society Morse Peckham has noted that the "high arts" have always been associated with and have validated the centers of power.⁹⁵ The modern illusion that artists must be honest with themselves and only serve, to borrow a phrase from Danton, "Truth—truth in all her rugged harshness" ignores the harshest truth, that artists survive only at the will of their patrons, customers, and audiences, and the powerful who sponsor the high arts have seldom been interested in truth for its own sake. The bohemian individual had no place in Renaissance Venice, and, with the exceptions of a few doges, the arts did not extol the uniqueness of the self, supporting personal oppositions to an inherited social role, as believed by those who equate the Renaissance with individualism, but protected the claims of society on the individual and offered an explanation for the existing order.

There were, of course, some changes in the political use of art in sixteenth-century Venice. The rise of pageantry, the acceptance of the antique style and iconography, and the interest in ingenious allegories that followed in the century after the War of the League of Cambrai reveal, if not a direct correspondence to the elite's quest for *nobiltà*, at least the emergence of a culture less accessible to the common Venetians than the old Venetian world of religious myth and communal civic values preached from every pulpit and in every ceremony. It is also clear that the interest in classical culture did not correspond in any way to the triumph of "bourgeois capitalism" but more likely to its rejection as a dominant value. Lastly, these changes in Venetian

⁹³ The interpretative framework and some of the phrasing here have been borrowed from Clifford Geertz's justly famous study, "Deep Play: Notes on the Balinese Cockfight," in his *The Interpretation of Cultures* (New York, 1973), 412–53, esp. 448. My interpretation has also been influenced by Erving Goffman, *The Presentation of Self in Everyday Life* (Garden City, N.Y., 1959).

⁹⁴ Geertz, *The Interpretation of Culture*.

⁹⁵ Peckham, *Beyond the Tragic Vision: The Quest for Identity in the Nineteenth Century* (New York, 1962). For comments on attempts to control the arts during the 1950s in America, see Jane De Hart Mathews, "Art and Politics in Cold War America," *AHR*, 81 (1976): 762–87.

state art may express a new sensitivity among the patricians to the notion that they could use the apparatus of the state to impose their own values and cultural proclivities on the entire society. This notion is, of course, a rudiment of what many historians call “modernization.”⁹⁶ What apparently governed the artistic transformation in Venice, then, was not so much an empirically verifiable change in economic or social conditions as it was the patricians’ perceptions of a change. Had the world-view the patricians inherited so colored reality that they could see things in no other way? Very little of their heroic, mythic past was evident in the reality of sixteenth-century Venice. That they felt insecure as a consequence was only natural. From their ancestors, however, they had also inherited a remedy for insecurity—myth-making. For them the manipulation of ever more brilliant images of power remained part of the pursuit of power itself.

⁹⁶ Marc Raeff, “The Well-Ordered Police State and the Development of Modernity in Seventeenth- and Eighteenth-Century Europe: An Attempt at a Comparative Approach,” *AHR*, 80 (1975): 1221–43. Also see Lane, *Venice: A Maritime Republic*, 252: “The contrasts between unrealized but not abandoned ideals on the one hand and attention to personal and group interests on the other hand made Venetian politics quite modern in spirit although quaint in details.”

Guild Republicanism in Trecento Florence: The Successes and Ultimate Failure of Corporate Politics

JOHN M. NAJEMY

THE FAMILIAR IMAGE OF REPUBLICAN FLORENCE as a political community has been dominated by ideas and assumptions largely inherited from the civic humanists of the fifteenth century, and in particular from Leonardo Bruni. This view of Florentine politics rests on two central notions: first, on the idea of the sovereign, centralizing state, the embodiment of the *res publica* and the locus of all political life; and, second, on the conviction that the operative components of this civil community were individual citizens to whom an equal degree of liberty was guaranteed by the state and from whom the exercise of active responsibility was expected within the state. For Bruni's contemporaries, these notions clarified and simplified the moral dimension of political life by focusing loyalty and responsibility in the abstract idea of a state that existed above parties and factions and that derived its legitimacy from the express or implied consensus of its politically active citizens.¹

For modern historians—both the believers in and the debunkers of the ennobling myths of Florentine republicanism—the assumptions of the civic

An earlier version of this paper was read to a joint session of the American Historical Association and the Society for Italian Historical Studies at the annual meeting of the American Historical Association in Washington, D.C., on December 28, 1976. I would like to thank the participants in that session—Anthony Molho, Werner Gundersheimer, and especially Gene Brucker, who commented on the paper with his customary expertise—for their helpful advice and suggestions. I am also happy to express my appreciation to Clive Holmes for his critical reading of the original draft and, as ever, to Hans Baron, always a generous teacher and friend, for his interest and encouragement.

¹ Bruni developed these ideas in a long series of writings from the *Laudatio Florentinae Urbis* (edited by Hans Baron in *From Petrarch to Leonardo Bruni* [Chicago, 1968], 232–63) to the monumental *Historiarum Florentini populi libri XII* (edited by E. Santini in *Rerum Italicarum Scriptores*, 19, pt. 3 [Città di Castello, 1927]) but perhaps nowhere more succinctly or effectively than in the funeral oration on Nanni Strozzi of 1428 and the *Vita di Dante* of 1436. On the oration, and the notions of political liberty and equality that it articulates, see Hans Baron, *The Crisis of the Early Italian Renaissance: Civic Humanism and Republican Liberty in an Age of Classicism and Tyranny* (rev. ed., Princeton, 1966), 418–30; and Nicolai Rubinstein, "Florentine Constitutionalism and Medici Ascendancy in the Fifteenth Century," in Nicolai Rubinstein, ed., *Florentine Studies: Politics and Society in Renaissance Florence* (Evanston, 1968), 446–47. In the vernacular *Life of Dante* Bruni lavished praise on the great poet for devotion to his political and civic duty in the years before his exile but criticized him for his harsh words against the government after he was banished in 1302. Bruni made it clear that even in exile, although wronged by his *patria*, Dante still owed the government loyalty and obedience. For Bruni's *Vita di Dante*, see Hans Baron, *Leonardo Bruni Aretino, Humanistisch-Philosophische Schriften* (Leipzig, 1928), 51–63; for the most recent English translation, see David Thompson and Alan F. Nagel, eds., *The Three Crowns of Florence* (New York, 1972), 57–73.

humanists have proved to be congenial territory. To the believers, they provide the comforting confirmation of the unity of theory and practice.² To the skeptics, they furnish the foil against which the cynical realities of patronage and elitist politics clearly emerge.³ Both camps have assumed that the alternative frames of reference for understanding Florentine politics consist in either the success or the failure of Bruni's twin notions. According to this view, the significance of the Florentine political experience in the two and a half centuries of the republic lies either in the successful realization of the goals of centralized state sovereignty and the consensus politics of participatory individualism or in their failure and the consequent affirmation of private and factional politics. Within the scope of these alternatives, historians of Florence have set out to measure the strength of the republic by gauging the extent to which the sovereign power of the state was or was not impaired by competing loyalties and allegiances⁴ and by assessing the size of the political

² See especially Baron, *Crisis of the Early Italian Renaissance*, *passim*, and "The Social Background of Political Liberty in the Early Italian Renaissance," *Comparative Studies in Society and History*, 2 (1960): 440-51. For some of those who have agreed, at least in part, with Baron that civic humanism was a genuine reflection of the realities of Florentine politics, see Anthony Molho, "Politics and the Ruling Class in Early Renaissance Florence," *Nuova Rivista Storica*, 52 (1968): 401-20; and Ronald G. Witt, "Florentine Politics and the Ruling Class, 1382-1407," *Journal of Medieval and Renaissance Studies*, 6 (1976): 243-67. For an optimistic assessment of the fortunes of republicanism in the wider Italian theater from the thirteenth to the sixteenth century, see Frederic C. Lane, "At the Roots of Republicanism," *AHR*, 71 (1965-66): 403-20.

³ The skeptics have generally stressed the power and self-interest of a more or less restricted oligarchy; see, *inter alia*, P. J. Jones, "Communes and Despots: The City State in Late-Medieval Italy," *Transactions of the Royal Historical Society*, 5th ser., 15 (1965): 71-96; Lauro Martines, *Lawyers and Statecraft in Renaissance Florence* (Princeton, 1968), 5, 204-05, 387-96; and Alberto Tenenti, *Firenze dal comune a Lorenzo il Magnifico, 1350-1494* (Milan, 1970), 23-24, 38-39, 94-98, 138-39. For a summary and critique of the views of the most unrelenting of the skeptics, Peter Herde, see Witt, "Florentine Politics and the Ruling Class," 244-46, 258-59, 265-66. Marvin Becker has taken a more complex view of the civic and republican mentality of early Renaissance Florentines, stressing the ambiguity of their commitment to these ideals. He has concluded that "the new paideia is as much a product of the reality as of the myth. . . . Myth and reality had blended, and while one could not be substituted for the other, each would lose meaning without its counterpart"; "The Florentine Territorial State and Civic Humanism in the Early Renaissance," in Rubinstein, *Florentine Studies*, 136-39. Nicolai Rubinstein has approached the position of the moderate skeptics in his belief that Bruni's emphasis on equality of opportunity in political life was "wishful thinking"; "Florentine Constitutionalism," 453. In general, however, Rubinstein takes a sanguine view of the strength and durability of "constitutionalist ideas" in Florence through the Medici period; *ibid.*, 455-62. Also see his more extended treatment of the subject in *The Government of Florence under the Medici* (Oxford, 1965).

⁴ See Martines, *Lawyers and Statecraft*, chaps. 4, 10. Martines has defined sovereignty in terms of the *majestas* of the *princeps* of Roman law and, for all practical purposes, has declared Florence a sovereign state because the city was, according to Bartolus's famous formula, *sibi princeps*. While acknowledging that in theory "all political power in Florence resided ultimately with 'the people'" and that the *populus florentinus* was the "wellspring of Florentine public law," he has, nevertheless, asserted that the "vital dynamic center of the Florentine political system was the executive—the Signory and the cluster of offices around it" (an idea quite in accord with the centralizing impulse of the civic humanist conception of sovereignty)—and that "it was to the Signory and its adjuncts, no less than to the vision of a *populus florentinus*, that they looked for the majesty and force associated with the *princeps* of the law books"; *ibid.*, 119-23, 422-25. Gene Brucker, writing of the fourteenth-century commune, says that it "was not a sovereign entity" because "its power was limited by entities which were almost completely exempt from its control, such as the church, or which possessed varying degrees of independence, such as organizations with recognized privileges and immunities: the Parte Guelfa, the guilds, the confraternities"; Brucker, *Florentine Politics and Society, 1343-1378* (Princeton, 1962), 57. Similar notions inform the analyses of Marvin Becker, who has seen the emergence of a powerful state in terms of the subordination of such entities to the supremacy of communal law; "The Florentine Territorial State," 110-16, 136-37. Of particular relevance to the theme developed in this essay is Becker's conceptualization of the decline of the guilds at the end of the fourteenth century in terms of the intrusion of "state authority" into "those bastions of the pluralistic medieval political universe"; *ibid.*, 115. Also see Becker's excellent early essay, "The Republican City-State in Florence (1280-1434)," *Speculum*, 35 (1960): 39-50. What is largely missing in these attempts to understand the nature and evolution of the Florentine republic is the idea that "state authority" and "sovereign" power, for some Florentines at least, were not incompatible with the active and even decisive participation in politics of some of those semi-independent "entities," namely, the guilds.

class through a counting of officeholders or eligibles.⁵ Both investigations have yielded useful and important results, but their application to this conceptual framework assumes only one kind of republicanism and one kind of participatory politics. Yet for a century and a half before Bruni's generation another kind of republicanism, based on the corporate politics of the guild system, was both a real and a controversial alternative in the world of Florentine politics.⁶

IN ITALY AS IN MUCH OF EUROPE, corporate and associative forms of organization proliferated during the thirteenth century: communes, guilds, parties, *consorterie*, confraternities, companies, and universities—all of them classifiable under the generic term of *universitates*.⁷ The phenomenon was spontaneous, complex, and infinitely varied, yet it generally tended toward the realiza-

⁵ See Molho, "Politics and the Ruling Class," 401–20; Witt, "Florentine Politics and the Ruling Class," 243–67; and Dale Kent, "The Florentine *Reggimento* in the Fifteenth Century," *Renaissance Quarterly*, 28 (1975): 575–638.

⁶ This is not to imply that the subject of corporate politics has never been treated in the historical literature on Florence. Among the historians who have made valuable contributions to this theme, the most distinguished is Alfred Doren, whose magisterial *Le Arti Fiorentine*, trans. G. B. Klein, 2 vols. (German ed., 1908; Florence, 1940) is the standard work on the Florentine guilds. The first and last chapters deal, respectively, with the emergence of the guild-based constitution in the thirteenth century and with the general problems of the relationship of the corporations to the state; *ibid.*, 1: 1–67, 2: 252–98. Also see Doren's earlier work, *Entwicklung und Organisation der Florentiner Zünfte im 13. und 14. Jahrhundert* (Leipzig, 1897). Briefer, but more incisive and conceptually more compelling on the specific problem of corporatism in Florentine politics, is the work of Franco Valsecchi, *Comune e Corporazione nel Medio Evo Italiano* (Milan, 1948–49), of which pages 96–145 are devoted to Florence; also see Valsecchi's *Le Corporazioni nell'organismo politico del medio evo* (Milan, 1931). Niccolò Rodolico has written much on the political history of the Florentine guilds in his many studies on the *popolo minuto*, the Ciompi revolution, and the guild regime of 1378–82. Of special interest for the general phenomenon of corporatism is his essay, "The Struggle for the Right of Association in Fourteenth-Century Florence," *History*, new ser., 7 (1922): 178–90. These and most of the other important contributions to the problems of Florentine corporatism, however, belong to a now distant generation of Italian and communal historiography. In the last thirty or so years, the guilds have been relatively neglected. Some good work has been done on one or another aspect of the history of the guilds; see, for example, Martines's analysis of the guild of the judges and notaries in *Lawyers and Statecraft*, 11–61, and Becker's study of the enforcement of antimonopoly legislation against the guilds, "La esecuzione della legislazione contro le pratiche monopolistiche delle arti fiorentine alla metà del secolo quattordicesimo," *Archivio Storico Italiano*, 117 (1959): 8–28. But there have been few and only partial attempts to evaluate the overall phenomenon of corporatism and the history of the guilds as a system or an ideology. Thus, in much recent writing on Florence the guilds have come to appear as so much excess baggage left from the Middle Ages, or as social clubs with little relevance to the real problems of Florentine politics and society. Since this paper was written and read to a session of the AHA's annual meeting in 1976, however, a significant exception to the general rule of the recent neglect of Florentine corporate politics has appeared in the form of Gene Brucker's *The Civic World of Early Renaissance Florence* (Princeton, 1977), with its arresting chapter on "Corporate Values and the Aristocratic Ethos in Trecento Florence." This is a bold attempt, and one with which I am largely in agreement, to explicate the "corporate spirit" and the political mentality of the guild community in the fourteenth century. Brucker has, in my view, quite rightly stressed the egalitarian dimension of corporate politics, the importance of corporatism in the Ciompi revolution and the popular regime of 1378–82, and the incompatibility of the corporate ideal and the hierarchical and elitist claims of the aristocracy. But his use of the concept of corporatism to embrace not only the guilds but also the family and the *gonfalone* departs significantly from the meaning and use of the term as developed in this essay. It is true that, in a general sense, guilds, families, neighborhood districts, and confraternities were all associations that provided for social cohesion and for the satisfaction of what Natalie Z. Davis has called "the appetite for belonging to and participating in a meaningful collectivity"; Davis, *Society and Culture in Early Modern France* (Stanford, 1975), 3. But, as this paper will try to suggest, the guilds, unlike these other associations, represented, in addition, a specific political and constitutional program, based on some definite legal and corporate principles, that carried the potential for a completely contrasting vision of the Florentine republic.

⁷ For a comprehensive view of the phenomenon to the mid-thirteenth century, see P. Michaud-Quantin, *Universitas: Expressions du mouvement communautaire dans le Moyen-Age Latin* (Paris, 1970), and "La Conscience d'être membre d'une universitas," in *Miscellanea Mediaevalia*, vol. 3: *Beiträge zum Berufsbewusstsein des Mittelalterlichen Menschen*, 1–14.

tion of the basic features of medieval corporation law within the framework of what Walter Ullmann has called the “ascending theory” of government.⁸ Each corporation was in essence the product of collective self-government by a body of equals. Many were based on some social, professional, or voluntary principle of selective inclusion. The legal principles used by the canon lawyers to explain the nature of ecclesiastical corporations in the thirteenth century were also applicable to the experience of the many secular corporations of late medieval Italy.⁹ A corporation was inherently the free creation of its members, acting voluntarily and in concert. The powers exercised in the name of the corporation were held to reside in the community of the members. The membership of the corporation, or a council representing the same, delegated those powers to elected heads, who served for limited terms and with the specific mandate to preside over the corporation, or to represent it, within the limits of its written constitution, according to the will of the majority, and for the common good of the community.¹⁰ The corporations acquired a legal personality and began to perform functions of a semi-public nature. Many of them, especially the communes and some organizations of merchants, also gained the power to make and enforce their own laws. Self-regulation and limited but increasing legislative power and jurisdiction were the first steps toward the assumption by medieval corporations of an autonomous political existence. To this point the development was principally internal and common to a variety of different corporations and organized *societates*.¹¹

The guilds of the Italian communes shared in this general process of corporate growth. In most cities there was a large increase in the number of guilds and a rapid extension of their political functions. With the upsurge in population and vigorous social mobility during the second half of the thirteenth century, wave after wave of new social and professional groups began to organize themselves in societies, confraternities, and guilds.¹² The political and institutional dimension of this social revolution, often referred to as the rise of the *popolo*, was firmly grounded in the principles of corporatism.

⁸ Ullmann, *A History of Political Thought: The Middle Ages* (Baltimore, 1965), 12–13, 159–61.

⁹ For the contribution of the canonists to corporation law, see Brian Tierney, *Foundations of the Conciliar Theory* (Cambridge, 1955).

¹⁰ For the model study of the affirmation and growth of a medieval corporation, see Gaines Post, “Parisian Masters as a Corporation, 1200–1246,” in his *Studies in Medieval Legal Thought* (Princeton, 1964), 27–60.

¹¹ For the development of medieval ideas on corporations from the realm of private law to public law and constitutional thought, see Otto Gierke, *Political Theories of the Middle Age*, trans. F. W. Maitland (Boston, 1959), 1–7, 37–73, *passim*; Brian Tierney, *Foundations of the Conciliar Theory*, and “Medieval Canon Law and Western Constitutionalism,” *Catholic Historical Review*, 52 (1966): 1–17; Gaines Post, “Roman Law and Early Representation in Spain and Italy, 1150–1250,” and “*Plena potestas* and Consent in Medieval Assemblies,” in *Studies in Medieval Legal Thought*, 61–162. On the problems of legislative autonomy, jurisdiction, representation, and delegation in corporate theory, see Michaud-Quantin, *Universitas*, 247–84, 305–26.

¹² For the rise of the *popolo* and the general environment of the corporate phenomenon in thirteenth-century Italy, see J. K. Hyde, *Society and Politics in Medieval Italy* (New York, 1973), 94–123; and D. Waley, *The Italian City-Republics* (New York, 1969), 164–200. The literature on the guilds of Italy is almost as extensive as that on the communes, although most of the comprehensive and comparative treatments of the subject are now quite old and out of date. Among the most valuable, see, in addition to the works of Franco Valsecchi, cited in note 6, above, Giovanni de Vergottini, *Arti e Popolo nella prima metà del sec. XIII* (Milan, 1943); and Alfred Doren, *Storia economica dell'Italia nel medio evo* (Bologna, 1965).

Logically enough, the strongest and best organized of these societies discovered that their greatest potential for political advancement lay in the direction of corporate federation. Through the political and sometimes military union of a large number of guilds under a common banner, the corporate model of political organization was, in effect, extended from the level of the single corporation, consisting of individual members, to the corporate federation, consisting of individual guilds. The same notions of corporation theory that had been applied to the experience of individual guilds were now applied to what was in essence a giant corporation of many guilds. The federation was the voluntary creation of its constituent parts; power and legitimacy derived from the community of corporations and from the express consent of the elected representatives of each participating corporation. Notions of delegation, representation, and consent regulated the life of the federation as they did that of individual corporations. Each guild within the federation was considered an equal and free member of the corporate community, just as the status of each individual member of a corporation or guild was held to be one of equality and autonomy vis-à-vis the other members. This picture is, of course, an idealized view of the political relationships of these corporate communities, but one that is found in their stated legal and constitutional principles. The creation of federations or unions of guilds, representing coalitions of new or newly prominent social classes, was achieved at different times during the course of the thirteenth century in a number of places including Padua, Bologna, Rome, Milan, and Florence.¹³ These federations produced something akin to the estates or orders that the corporatist school has found elsewhere in Europe in roughly the same period.¹⁴ The *pars populi*, or *comune populi*, institutionalized in the corporate federation, took its place within and sometimes next to the *comune maius*, or original commune, as one of the constituent orders of urban society.¹⁵

¹³ For Padua, see J. K. Hyde, *Padua in the Age of Dante* (New York, 1966), 243. The union of the Paduan guilds was effected by 1293 and stipulated that "all and each of the guilds of the city of Padua . . . may be understood to be and are from henceforth a single body, society, brotherhood or league to maintain and conserve the city of Padua and its district in a peaceful state as a commune, free from the domination of any tyrant or any other single person." In Bologna the federation of the guilds was apparently a part of the constitution of the *primo popolo* of 1228. In 1233 the guilds renewed their formal agreement, which declared that the "societates armorum et artium que sunt sub ancianis sint unum et idem, et idem sacramentum faciant; et ad electionem ancianorum vadant omnes armorum et artium"; Valsecchi, *Comune e Corporazione*, 84. For a more detailed discussion, see de Vergottini, *Arti e Popolo*, 15-49. In Rome the guilds were never as powerful as in Florence, Bologna, or Padua, but they still managed to achieve a federation of thirteen corporations in the *universitas mercatorum*, or *mercantantia*; Valsecchi, *Comune e Corporazione*, 187. For Milan, it is uncertain whether the famous *Credenza di Sant'Ambrogio* of 1198 was actually a federation of previously constituted guilds; Valsecchi seems to believe that it was; *ibid.*, 53-54, 69, 211. De Vergottini is, however, of the contrary opinion; *Arti e Popolo*, 73-74. For Florence, see below.

¹⁴ Helen M. Cam and Antonio Marongiu, "Recent Work and Present Views on the Origins and Development of Representative Assemblies," in *Relazioni del X Congresso Internazionale di Scienze Storiche*, vol. 1: *Metodologia, Problemi Generali* (Florence, 1953), 3-75. Also see the rich collection of studies on corporatism and representative estates in the volumes of the *Études Présentées à la Commission Internationale pour l'Histoire des Assemblées d'États*, and in particular the contributions of Émile Lousse, "La Formation des ordres dans la société médiévale," in *L'Organisation corporative du Moyen Âge à la fin de l'Ancien Régime*, 2 (Louvain, 1937): 61-90; and Georges de Lagarde, "La structure politique et sociale de l'Europe au XIV^e siècle," in *ibid.*, 3: 93-118.

¹⁵ Valsecchi, *Comune e Corporazione*, 16.

IN FLORENCE, THE GUILDS DID NOT CONTENT THEMSELVES with achieving the condition of an "estate" or "order" within the commune. They proceeded instead to a third stage in which the very sovereignty of the commune was absorbed by the corporate federation itself.¹⁶ The aim of the guild movement in Florence was to achieve the greatest possible degree of association between the commune and the corporate federation. To the extent that this was achieved, it became possible and, indeed, necessary to apply to the organization of Florentine communal government those same basic principles of corporation law that had been nourished on the level of single corporations and that had subsequently been elaborated within the framework of the guild federation. The affirmation of these corporate principles of political organization at the level of communal government produced what can be called "guild republicanism."

Guild republicanism first became a reality in Florence during the 1290s. The creation in 1282 of a new chief magistracy of communal government called the "Priors of the Guilds" was a preliminary step (the very name of the new office suggests the merging of the legal identities of the commune and the guild federation), but not until 1292 did the adherents of the guild movement succeed in implementing a specifically corporate approach to the election of the priorate. In the electoral debate of November 1292, which marks the real beginning of the popular government based on corporate principles, it was decided that the new Priors would be elected according to a system that called for independent nominations from each of twelve guilds and that allowed no more than one Prior from any guild in each term of office.¹⁷ Inherent in this apparently simple innovation in electoral procedure were two ideas of potentially revolutionary significance: first, that each of the guilds would participate, through its Consuls, autonomously and on an equal basis with the other guilds, in the electoral process; and, second, that, as far as possible, the guilds were to be evenly represented in the priorate.¹⁸ In other words, the traditional corporate principles of equality and autonomy among participating members of a corporation were written into the procedure for electing the highest executive magistracy of the commune. This meant that the guilds of furriers, butchers, shoemakers, iron-workers, master-builders, and used-cloth dealers would participate on equal terms in these elections with the guilds of bankers, woolen cloth manufacturers, and international traders.

The Ordinances of Justice of January 1293 gave formal constitutional expression to the vision, implied in the outcome of the November debate, of the Florentine commune as a sovereign federation of equal and autonomous guilds, and it did so in unmistakably corporate language. The opening lines of the first rubric, which established the legal framework for the union of the twenty-one guilds in their "good, pure and faithful society and company,"

¹⁶ *Ibid.*, 138.

¹⁷ Alessandro Gherardi, ed., *Le Consulte della Repubblica Fiorentina dall'anno MCCLXXX al MCCXCVIII*, 2 (Florence, 1898): 226.

¹⁸ The evolution of Florentine electoral politics from 1280 to 1393, with particular attention to the fortunes of the corporate approach to the election of the priorate, is the subject of a monograph that I hope to publish in the near future.

echo the famous dictum of Roman law, *quod omnes tangit*, which was the basis of many attempts during the late Middle Ages to understand the nature of representative and corporate systems of government.¹⁹ “That is judged to be most perfect which consists of all its parts and which is approved by the consent of them all. . . .”²⁰ The “parts” were, of course, the guilds themselves, and this opening clause forcefully asserted that the legitimacy of the corporate federation (which, according to these same Ordinances, was now entrusted with both the election and the defense of the central magistracy of communal government) had henceforth to derive from the consent of the equal partners of the corporate union. The decentralized sovereignty of the guild federations and the interposition of the corporations between citizens and commune effectively made the guilds the vehicles for the expression and interpretation of the popular will.

To appreciate fully the implications of this constitutional arrangement, it is useful to have some idea of the size of the guild community that was called upon to participate in the federation. Working from some precise matriculation figures and a few hopeful guesses, a reasonable estimate for the period around 1300 would be in the range of seven to eight thousand matriculated members of the twenty-one guilds, with perhaps fifty-five hundred to six thousand in the top twelve guilds.²¹ In a population of about one hundred

¹⁹ On *quod omnes tangit* and its political uses, see Gaines Post, “A Romano-Canonical Maxim, *Quod Omnes Tangit*, in Bracton and in Early Parliaments,” in Post, *Studies in Medieval Legal Thought*, 163–238. Also see Antonio Marongiu, *Medieval Parliaments: A Comparative Study*, trans. S. J. Woolf (London, 1968), 33–37; and Yves M.-J. Congar, “*Quod Omnes Tangit*, ab omnibus tractari et approbari debet,” *Revue historique de droit français et étranger*, 4th ser., 36 (1958): 210–59. For the application of the maxim in corporation theory, see Michaud-Quantin, *Universitas*, 283–84, 324–26. Michaud-Quantin has pointed out a basic contradiction between the idea of direct consultation of all of the interested parties, as *quod omnes tangit* was understood, and the notion of representation that had been developed in corporation theory; *ibid.*, 325. However valid this observation might be for the problem of the nature of consent *within* a corporation, where the consultation of every member was not possible and consequently where the maxim was in a strictly theoretical sense inapplicable, it is clear that such a contradiction did not exist in the case of a corporate federation in which it was eminently possible to consult directly all of the guilds. The notion of representation was, of course, fundamental to the practice of appointing syndics to take the oath on behalf of each of the guilds. See note 20, below.

²⁰ For the text of the Ordinances of Justice, see Gaetano Salvemini, *Magnati e Popolani in Firenze dal 1280 al 1295* (Florence, 1899), 384–432. The section that contains the translated passages reads as follows: “*Quoniam illud perfectissimum approbatur quod consistit ex omnibus suis partibus et omnium iudicio comprobatur; ideo . . . ordinatum et provisum est quod duodecim maiores Artes . . . et etiam omnes alie infrascripte Artes civitatis Florentie . . . et artifices ipsarum Artium, quarum et quorum presidio certum est civitatem et Comune Florentie defensari, debeant et teneantur Sindicos ydoneos et sufficienter instructos ad omnia et singula infrascripta constituere legitime . . . Qui Sindici . . . sibi ad invicem promittant facere et curare quod Artes . . . et homines ipsarum Artium facient et observabunt bonam et puram et fidelem societatem et compagniam. . . . Et promittant inter se ad invicem solempniter nullas coniurationes promissiones obligationes vel posturas vel conventiones aut iuramenta facere vel servare inter se nisi hanc presentem societatem et compagniam, sacramentum et unionem universalem inter omnes ipsas Artes ut predicatur iniendam”; *ibid.*, 385–88. On the appointment of syndics and proctors who were *ydonei* and *sufficienter instructi*, see Post, “*Plena Potestas and Consent*,” 127–60.*

²¹ Some of the guilds were extraordinarily large in this period. The *matricula* of the guild of judges and notaries for the year 1280 lists 65 judges, 374 notaries of the city, and 204 notaries of the *contado*, for a total of 643 members; Archivio di Stato di Firenze [hereafter ASF], *Arte dei Giudici e Notai*, 5, ff. 50–62v. A list “*extracta . . . ex nova matricula*” for the same guild in 1338 contains 946 names; *Giudici e Notai*, 21, ff. 1–36. The 1332 *matricula* of the wool guild lists 613 *lanifices*, *stamfices*, and *lanvendioli*; *Arte della Lana*, 19, ff. 1–4, 25–26, 49–50v, 73–74. The bankers’ guild included some 355 members in its *matricula* of 1300, 321 in 1301, and 271 in 1302; *Arte del Cambio*, 6, 7, 8. The guild of Por Santa Maria had 634 active members in the year 1320; *Arte della Seta*, 6, ff. 57–65. A fifteenth-century copy of the names contained in the early *matricule* of the guild of the doctors and druggists lists 508 persons for the year 1297, 223 for 1301, and 553 for 1312; *Arte dei Medici e Speziali*, 7. The two remaining major guilds were smaller. In 1237, 207 persons took the oath of obedience to

thousand, the guild community thus included more than a third of the adult males of the city. What made the new arrangement revolutionary in terms of the social bases of politics was that these guilds represented the interests of specific professional and occupational groups. The corporate formula for attaining the consent upon which the legitimate exercise of the government's power was now based guaranteed an equal voice to each of the participating guilds and thereby institutionalized the representation of a wide range of social groups with varying economic and class interests.

Now this is admittedly a "pure" interpretation of a point of constitutional theory. Obviously, the realities of Florentine politics were not fully consistent with these principles, even under the popular government of Giano della Bella. But it does not follow that no one took such ideas seriously or that they were never implemented in the political arena. One dramatic application appears in the chronicle of Dino Compagni and involved Compagni's own role in the central and most crucial episode of the civil conflict between the Black and White Guelfs in the opening years of the fourteenth century. Compagni, who had been a loyal partisan of the popular government of the mid-1290s, was elected one of the new Priors of October 1301—at a time when the White party was striving to keep out of the city the French prince, Charles of Valois, who had come into Tuscany in support of the Blacks. Compagni agreed that no good would come from permitting Charles to enter Florence, but, "wishing," as he later wrote in the *Cronica*, "to do nothing without the consent of their fellow citizens," he and the other Priors made, in the waning hours of their abbreviated term of office, an appeal to the guilds of the city. They asked "each guild to submit a written opinion stating whether it wished and approved that Charles of Valois be permitted to enter Florence as a peacemaker."²² The appeal failed (only one guild declared itself opposed to

the Consuls of the Calimala, and nearly a century later the guild had not changed appreciably in size, listing 196 names in its *renovatio matricule* in 1328; *Manoscritti*, 542, no pagination. The furriers numbered 194 according to a matriculation list of 1317; *Arte dei Vaiai e Pellicciai*, 9. The five middle and nine minor guilds were mostly smaller than the major guilds, ranging in membership from under 100 to about 300, with two significant exceptions: the master-builders and the shoemakers. For the master-builders (carpenters and masons) an incomplete *matricula* of 1358 lists 352 names; *Arte dei maestri di pietra e legname*, 1, ff. 1–29v. The guild must, in fact, have had upwards of 450 members even at this postplague date, and perhaps many more a generation earlier. But the most astounding of all the matriculation statistics comes from the guild of the shoemakers. Even Villani's comment that "the shops of shoemakers, slipper makers, and wooden-shoe makers were so numerous they could not be counted" hardly prepares one for the incredible list of 1557 shoemakers of the city and 1294 shoemakers of the *contado* in the guild's *matricula* of 1313; *Arte dei Calzolai*, 2, ff. 1–37v; for Villani's comment, see R. S. Lopez and I. W. Raymond, *Medieval Trade in the Mediterranean World* (New York, 1968), 72–73. The size of the guild community and the problems surrounding the issue of matriculation (including the political significance of the *matricule*) will be treated in detail in a study on the Florentine guilds now in preparation.

²² Compagni, *La Cronica di Dino Compagni*, ed. I. Del Lungo, in *Rerum Italicarum Scriptores*, 9, pt. 2 (Città di Castello, 1913): 95–96: "I Signori dissono agli anbasciadori, risponderrebbero al loro signore per anbasciata; e intanto preson loro consiglio: perchè, essendo la novità grande, niente voleano fare senza il consentimento de' loro cittadini. Richiesono adunque il Consiglio generale della Parte guelfa e delli LXXII mestieri d'Arti, i quali avean tutti consoli, e inposono loro, che ciascuno consigliasse per scrittura, se alla sua arte piaceva se messer Carlo di Valos fosse lasciato venire in Firenze come paciaro. Tutti risposono, a voce e per scrittura, fusse lasciato venire, e onorato fusse come signore di nobile sangue: salvo i fornai, che dissono che nè ricevuto nè onorato fusse, perchè venia per distruggere la città." How there could have been seventy-two politically recognized guilds ("i quali avean tutti consoli") is a problem that will be treated elsewhere, but it does not affect the significance of Compagni's decision to appeal to each of the corporations on an equal basis.

the arrival of the French prince), but the important point is that this formal consultation of individual guilds was Compagni's idea of the way to obtain the "consent of his fellow-citizens." His gesture to the guilds was entirely consistent with the constitutional principles contained in the Ordinances of 1293.

This complex of ideas about the nature of politics in a commune ruled by a corporate federation did not die in Florence with the failure of that first guild government. It survived—fitfully, unevenly, and in an increasingly radical form—through most of the fourteenth century. The guilds, of course, remained, and so, in theory at least, did the corporate union of the twenty-one guilds. But, as a matter of policy, the oligarchic governments did not stress the close constitutional association of the commune and the corporate federation. These governments sought to disengage the electoral process from the corporate principles of the guild system and to base the legitimacy of government and the consent of the community on grounds other than those of the decentralized sovereignty of the guilds. Although these aims were definitively achieved by the end of the fourteenth century, guild republicanism did not die easily. On a number of occasions during the century, this alternative conception of the Florentine body politic reappeared in the political arena as the constitutional claims of the corporate system were reasserted. Two of these occasions are worth looking at in some detail.

One of the most striking instances of the revival of Florentine corporatism occurred in the crisis of the 1340s. The circumstances are well known: oligarchic government was severely compromised by military failures in 1341, by the disastrous appeal to a foreign tyrant in 1342–43, and by the impending collapse of several of the largest banking companies.²³ In the fall of 1343, the proponents of guild government resurrected the ideas of 1293 with a new and more radical formula: "the Florentine Republic," it was asserted in at least two official documents of these years, "is ruled and governed by the guilds and guildsmen of the same city, and especially by the Consuls of those guilds. . . ."²⁴ In this formula the verbs *regitur* and *gubernatur* themselves connote sovereignty, and the *res publica*, so dear to the civic humanists of the fifteenth century, is here made to coexist with and, indeed, is "ruled and governed" by those most unclassical institutions, the guilds. There is, moreover, a remarkable parallel between this self-definition by the new popular regime and the description of it by Giovanni Villani, who excoriated this government for its dependence on "guildsmen, manual laborers and illiterates, since most of the Consuls of the twenty-one guilds, by whom the commune was then being ruled, were lowly guildsmen. . . ."²⁵ Villani's echo of the formula for corpo-

²³ On these years, see Marvin Becker, *Florence in Transition*, 2 vols. (Baltimore, 1967–68), 1: 123–76.

²⁴ ASF, *Provisioni, Registri*, 32, f. 83v (December 4, 1343): "Considerantes quod Florentina res publica per artes et artifices civitatis eiusdem regitur et gubernatur, et precipue per Capituldines ipsarum Artium. . ."; and, again, in *Capitoli, Protocolli*, 4, f. 113 (September 20, 1345): "Domini Priores Artium et Vexillifer Iustitie supradicti avide affectantes quod florentina res publica que precipue per artes et artifices civitatis Florentie regitur et gubernatur diligenter sollicite et actente et cum magna providentia gubernetur. . . ."

²⁵ Villani, *Cronica di Giovanni Villani*, ed. Franc. Gherardi Dragomanni, 4 vols. (Florence, 1845), 4: 75: "E nota, che fa il reggimento delle città, essendone signori artefici e gente manovali e idioti, perocchè i più delle ventuna capituldini dell'arti, per le quali allora si reggeva il comune, erano artefici minuti venuti di contado o forestieri, a cui poco dee calere della repubblica, e peggio saperla guidare."

rate sovereignty, coming as it does from a harsh critic of the regime, is a compelling confirmation of the popular government's own conception of its constitutional order.

How accurate were these claims? Perhaps the most dramatic example of the expansion of guild power and the impact of corporate politics in these years can be seen in the role that the guilds played in the settlement of major bankruptcies. One of the prime objectives of the guild regime that came to power in the fall of 1343 was to establish firm control over the institutions which would be called upon to deal with these complex litigations and especially to resolve the conflicting claims of foreign creditors and appeals for delay from hard-pressed bankers and merchants who hoped to shift the burden of the claims against them onto their own debtors. Chief among these institutions was the Mercanzia, which had been founded in the first decade of the fourteenth century to deal with a similar banking and credit crisis.²⁶ The Mercanzia was an oligarchic institution dominated from its inception by the international bankers and merchants of the five major commercial guilds (Calimala, Cambio, Lana, Por Santa Maria, and Medici e Speciali). The Mercanzia's five governing councillors (one from each of the five guilds) had traditionally used their power to protect this commercial oligarchy and its vital economic interests. It was to the Mercanzia that the petitions of creditors came and from which the threatened bankers expected a sympathetic hearing. But the government of the guilds had other plans for the Mercanzia: in July 1344 an *ad hoc* Committee of Twelve was created and joined to the five councillors. Special legislation authorized the Twelve and the Five, sitting together as a single body, to administer the settlement of creditors' claims through the seizure, distribution, and sale of the goods and assets, including land, of the bankrupt merchants.²⁷

The popular government, in short, steered the banking crisis toward a solution that meant disaster for the great companies by allowing the claims of creditors to be satisfied directly through the liquidation of the enormous landholdings that served as the indispensable security in the high-risk deposit banking business. The joint committee of the Five and the Twelve became the ultimate arbiter in this process at a time when the greatest banking companies of Florence were collapsing. It was thus a matter of enormous importance that the all-powerful Twelve (who could, of course, out-vote the Five and were essentially in control of the situation) were elected by the Consuls, not of the five guilds of the Mercanzia, but of all twenty-one guilds of the corporate federation.²⁸

Nor was this the only application of the principles of Florentine corporatism in the bankruptcy crisis. Early in 1346, as the fate of the powerful Bardi company was about to be decided, the Consuls of the bankers' guild held a council meeting to discuss what advice the guild should give to the government "concerning the assets of bankrupt merchants." The government, in the

²⁶ On the origins and growth of the Mercanzia, see G. Bonolis, *La Giurisdizione della Mercanzia in Firenze nel secolo XIV* (Florence, 1901); and, for the fundamental study, Armand Grunzweig, "Les Origines de la Mercanzia de Florence," in *Studi in Onore di Gino Luzzatto*, 1 (Milan, 1949): 220-53.

²⁷ ASF, *Mercanzia*, 3, ff. 86-87.

²⁸ *Ibid.*

spirit of Dino Compagni's appeal to the guilds in 1301, very likely requested such advice from the bankers' guild and perhaps from other guilds as well. In any case, the result of this open debate in the guild's "representative assembly" was a recommendation that officials of the commune be empowered to seize the landholdings of bankrupt merchants and to prevent them from being used or inhabited until the claims of creditors had been satisfied according to stipulated agreements.²⁹ The details and the outcome of this policy of the guild government must await further study, but the important point here is that the popular government, facing the greatest economic crisis in the history of the Florentine merchant oligarchy, calmly formulated a policy based on the direct consultation of at least some sectors of the guild community and carried out that policy through a committee that was elected and directed by the Consuls of all twenty-one guilds. It hardly needs to be added that the Bardi were indeed ruined.³⁰

From this specific instance of guild government, it becomes easy to understand why, in the minds of many prominent Florentines like Giovanni Villani, who agonized over the destruction of the Bardi,³¹ the whole idea of government by or through the guilds began to be very distasteful. Villani had no quarrel with the Ordinances of Justice as the foundation of a convenient and patriotic historical myth. But the actual implementation of that document's constitutional principles in the delicate circumstances of the 1340s was another matter altogether. It was madness, he believed, to expect that lowly guildsmen, from each and every one of the twenty-one guilds, could properly care for the affairs of the republic.³² The guild idea had been revived at a time when the economic survival of the Florentine oligarchy was in doubt, and what Villani accurately perceived was that the implementation of corporate principles at such a time was bringing Florence dangerously close to a condition of class conflict in which his own class was at a distinct constitutional disadvantage. For, in any arrangement in which the twenty-one guilds spoke with twenty-one equal voices on public issues, the oligarchy was bound to be hopelessly outnumbered. The dangers for the oligarchy inherent in the principles of 1293 began to be understood, and one of the first acts of the restored oligarchic regime of 1348 was to reduce the number of minor guilds from fourteen to seven through the consolidation of several corporations.³³ This attempt to deprive the nonoligarchic guilds of their numerical advantage in the corporate federation lasted only two years.³⁴ But the fears engendered by the revival of corporate principles of government in the 1340s were never allayed in the minds of Florentine merchants and bankers. The alienation of the Florentine upper classes from the ideals of their own corporate past was crucial to the ultimate failure of guild republicanism.

²⁹ *Arte del Cambio*, 57, ff. 5-5v.

³⁰ For a somewhat different approach to the resolution of the banking crisis, see Armando Saporì's *La Crisi delle Compagnie Mercantili dei Bardi e dei Peruzzi* (Florence, 1926).

³¹ Villani, *Cronica*, 4: 92-93.

³² See note 25, above.

³³ ASF, *Manoscritti*, 269, no pagination.

³⁴ For the text of the provision of October 12, 1350 that restored the traditional number of twenty-one guilds, see Niccolò Rodolico, *Il Popolo Minuto* (reprint ed., Florence, 1968), 110-11.

THE INCREASING RADICALIZATION OF THE GUILD MOVEMENT in that same decade of crisis also contributed to the alienation of the upper classes from the assumptions of corporate politics. As a result of the bankruptcy settlements and the early legislation on the funded public debt,³⁵ government by the guilds was now associated with an openly anti-oligarchic stance on economic issues. But the movement was radicalized in another and perhaps more profound sense in these years. The notion of the guild community as a voluntary federation of equal and autonomous members tended, in the nature of things, to be an expansive ideal. Popular governments often needed support for their constitutional and political programs and were most likely to find it lower down in the social and professional hierarchy among those groups willing to trade such support for recognition of their own corporate and political aspirations. Although the oligarchy was beginning to recoil from the idea, the constituencies of the established guilds had, in fact, set an important example by embracing the idea of corporate organization as the most effective means for safeguarding their class interests.

This example was not lost on those artisan and working elements of Florentine society that did not have their own legally recognized guilds. Thus, in the 1340s, expansionary tendencies of two kinds appeared in the guild movement: not only did effective political power within the established guild community become more evenly shared (again and again, in both the official documents and in the testimony of Villani, the Consuls of all twenty-one guilds are described as the arbiters and agents of political decisionmaking), but also groups outside the guild community attempted to establish their own corporations and to join the federation. As early as the fall of 1342, the dyers of the textile industries petitioned Walter of Brienne for a guild of their own to be equal in standing to the established guilds.³⁶ In 1345 the government dealt harshly with Ciuto Brandini, the leader of a workers' movement that aimed at the creation of a corporation—a union, we would say—in defense of their interests.³⁷ He was executed by a government that wrapped itself in the principles of corporatism, for the crime of having attempted to establish a corporation for workers. For the upper ranks of the guild community, which felt threatened by such attempts, class interests and constitutional principles began to veer apart. The growing hostility of the established guilds toward the

³⁵ The popular government at first suspended interest payments, consolidated all outstanding debts, and tried to amortize the principal. Then in 1345, realizing that full amortization was impossible, the government renewed the payment of interest, but at only 5 percent, far less than had been anticipated by the more affluent creditors of the commune; on the *Monte*, see Becker, *Florence in Transition*, 2: 151–200.

³⁶ They asked to be recognized as “unum et idem corpus, universitas et collegium . . . , et quod deinceps non cogantur nec cogi possint per aliquam universitatem aliquarum Artium civitatis Florentie, . . . nec adstricti vel suppositi sint vel possint esse sub earum iurisdictione; sed generaliter possint et eis liceat facere exercere et ordinare ea et singula que possunt alie universitates Artium et collegiorum dicte civitatis. . . .” For the text of the petition, see Cesare Paoli, *Della Signoria di Gualtieri Duca d'Atene* (Florence, 1862), 82–83.

³⁷ He was accused of instigating the organization of workers in the wool industry “in maiori numero quantum habere posset” into “quedam fraternitas,” with the regular appointment of Consuls and the collection of dues “ut fortiores et duriores essent in dicta iniqua societate. . . .” For the text of the condemnation, see Rodolico, *Il Popolo Minuto*, 102–03; and, for a translation, see Gene Brucker, *The Society of Renaissance Florence: A Documentary Study* (New York, 1971), 235–36.

corporate aspirations of those without guilds seriously weakened the long-term prospects of guild republicanism. The guild movement began to discover that it was alienating both the upper and the lower ranks of Florentine society. Caught in an unstable position between the oligarchy, which despised the corporate system of government, and the lower classes, which desired nothing more than to become a part of it, the guild movement was increasingly isolated and on the defensive.

Amid these internal tensions and unresolved contradictions the theories of guild government were revived in the long social crisis of the 1370s and early 1380s. The reaffirmation of corporate principles began as early as 1370 in the wool guild. In opening the election of the guild notary in that year to the full membership of the guild's council, the proponents of reform resurrected a classic formula: "it is just and consonant with law," they asserted, "that this election be held by the guild and its members, since *that which touches all must be approved by all*."³⁸ The use of the famous maxim demonstrates that the issues of guild politics were still conceptualized within the legal and ideological framework of corporation theory. The maxim was, in fact, the keynote of a broad effort to make the internal politics of the guild more democratic by reducing the *balia* powers of the Consuls and expanding the role of the council.³⁹ These reforms culminated in January 1374 with a provision that made it obligatory for the Consuls to hold regular meetings of the guild council at least twice in each consular term of four months: a sort of secular anticipation of the famous decree *Frequens* of the Council of Constance. But it is the prologue of this provision that sums up in the most optimistic terms the basic assumptions of corporate politics underlying this renewal of the ideals of consent and representation in the guild:

Whenever it happens, as a result of some compelling necessity, that the Consuls of this guild convoke and assemble good and expert members of the guild in the guildhall, and seek counsel and confer with them on matters of utility and benefit to the guild, with the assembled members of the guild continuously consulting and talking with each other in these assemblies and meetings, the result is that many issues are raised and brought to the attention of the Consuls of the guild, which redound to the honor, utility, and convenience of the guild and its members.⁴⁰

Regular meetings and spontaneous consultation among the members, faith in the collective wisdom of the members to discern the common good, initiatives emerging from such exchanges to mold the policies of the leadership—such

³⁸ *Arte della Lana*, 45, f. 55 (January 21, 1370): "Considerantes quod iustum et iuridicum est dictam electionem . . . fieri per artem predictam et artifices dicte artis quia quod omnes tangit debet ab omnibus approbari . . ."

³⁹ *Ibid.*, f. 55v.

⁴⁰ *Arte della Lana*, 46, f. 8 (January 25, 1374): ". . . Considerantes quod quandocumque accidit necessitate cogente Consules dicte artis convocare et coadunari facere in domo dicte artis bonos et expertos artifices dicte artis, et cum eis consulere et conferre de commodis et utilitatibus dicte artis, et continuo in huiusmodi coadunationibus et consiliis, artificibus dicte artis adinvicem colloquentibus et consultantibus, multa conferuntur et consulibus dicte artis recordantur que resultant ad honorem utilitatem et commodum artis predicte et artificum ipsius artis. . . ."

were the basic ideals of guild republicanism and of the ascending quality of corporate government. The implementation of these ideals within the community of the Florentine guilds during the decade of the 1370s enabled the guilds to become the focal points of initiative, opposition, and ultimately revolution in 1378.

The revival of corporate principles within individual guilds was paralleled in these years by their reaffirmation at the level of the corporate federation. Sometime in the summer or fall of 1378, an advisory committee of twelve citizens submitted to the Signoria a long list of recommendations, or “ricordi,” dealing with the most difficult and sensitive issues facing the government. At the end, the authors reminded the Signoria that in turning these proposals into legislation they must not forget to consult with the *Capitudini*, or Consuls, of the guilds:

Finally, it seems to us that, if it seems fitting to you, all of the aforementioned matters should be brought up and discussed with the Consuls of the guilds, and *their advice on these questions should be requested*; so that, if all or part of these proposals become law, it will have been done with the agreement and consent of the guild Consuls; *and then it can truly be said that it has been done with the consent of the whole city.*⁴¹

Here again, in a most revealing formulation, is the fundamental assumption that the exercise of communal sovereignty was ultimately dependent upon the consent that could only be obtained from a consultation of the guild community. Such consent was, moreover, equivalent to the consent “of the whole city.” The implication is clear that, without the approval of the Consuls of the individual guilds of the corporate federation, the enactment of the committee’s proposals would be vulnerable to the charge that they did not enjoy the legitimizing support of the popular will.

In the circumstances of 1378, of course, the consent of the guild community came closer to the consent of the “whole city” than it did in more normal times. The revolution of that summer resulted in the expansion of the corporate federation to include three new guilds, two for the skilled artisans of the textile industries and one for the unskilled Ciompi. The reassertion of corporate principles of government both within the guilds and within the federation was accompanied by an extension of those principles to a wider range of social groups. From this perspective, the events of 1378 may reasonably be viewed as a “guild revolution” and as the last serious attempt by Florentines to construct a polity on corporate principles.

The government that emerged from this revolution prevailed for another three and a half years. How it viewed itself can be seen in a *missiva* of 1380, written by the chancellor, Coluccio Salutati, concerning the conspiracy of a band of exiled Guelfs against the regime. Among the crimes planned by these

⁴¹ ASF, *Capitoli, Protocolli*, 7, f. 190v: “Apresso ci pare che parendo a voi che tucte le predette scripture di sopra si debbano conferire e ragionare colle capitadini, e a loro di cio domandare consiglio, per cio che se tucto o parte dele dette cose si delibera, sia di concordia e di contentamento loro; il quale contentamento si potra dire che veramente sia di tucta la citta.”

exiles, he wrote, were murder, torture, arson, and the destruction of “the most honorable corporations of the guilds of our city, *through which, by the grace of God, we are what we are*, and without which, should they ever be suppressed, the very name of the Florentines would without doubt be erased from the face of the earth.”⁴² *Per quas sumus quod sumus*: through which we are what we are—half a generation later this would certainly have seemed an extravagant and almost incomprehensible claim, especially from a chancellor of the republic. But for the Salutati of 1380, working for a government that based its legitimacy on the corporate principles of guild republicanism, it was perhaps only natural to proclaim that the very identity of the Florentines and the reputation of the city were bound up with the survival and prosperity of their guilds. That a humanist chancellor of the republic could have uttered such sentiments may serve as testimony to the extraordinary power of the guild idea as public policy in those years.

WHY, THEN, DID THE IDEA FAIL? Aspects of the answer have already been suggested. The guild republicanism of the Florentine corporate community was, to be sure, an expansive but never a universal ideal. Although the guild constitution had been built on the fundamental right of corporate association, the established guilds denied this same right to the lower orders of Florentine society. Guild republicanism might have succeeded as a socioconstitutional system if its basic principles had been allowed to extend far enough. Paradoxically, the success of the guild idea in 1378 assured its ultimate failure. The Florentine upper classes were profoundly frightened by the social radicalism of the guild revolution of 1378–82, and the impact of the experience cast a long shadow over the next generation. Bankers, manufacturers, and international merchants, realizing that the principles of the corporate state were an ever-recurring danger to their own security and hegemony, began to see the guilds in a different light; no longer were the guilds considered central to the Florentine political system or to its historical traditions.

Chroniclers and historians who reflected the thinking of the upper class now treated the guilds with indifference or contempt. Whereas Giovanni Villani had written in some detail about the guilds and their part in the creation of the priorate in 1282,⁴³ he had much less to say about their far more decisive impact on Florentine politics in the 1290s.⁴⁴ Perhaps he did not wish to emphasize that portion of the history of the guilds that provided an important

⁴² ASF, Signori, Carteggi, Missive, I^a Cancelleria, 18, f. 108v: “. . . coniuraverunt in nostre urbis excidium, ordinantes, civitatem incendere, et ferro in concives suos, viros equidem optimos, inauditam seviciam crudeliter exercere. Ordinabant etiam artium nostre civitatis per quas post dei gratiam sumus quod sumus, quibusne sublati florentinorum nomen a facie terre procul dubio tolleretur, honestissima delere collegia, et totam civitatem artificum innocenti sanguine deformare. Deus autem optimus, benignus et pius, tante iniquitatis consilia dissipavit.” On this letter, see Eugenio Garin, “I cancellieri umanisti della Repubblica fiorentina ca Coluccio Salutati a Bartolomeo Scala,” in *La Cultura Filosofica del Rinascimento Italiano* (Florence, 1961), 3–18.

⁴³ Villani, *Cronica*, 1: 410–11.

⁴⁴ *Ibid.*, 2: 5–7.

precedent for the corporate politics of the 1340s, of which, as we have seen, he was so critical. Although his brother Matteo largely ignored the guilds, his chronicle covers a fifteen-year period (1348–63) in which the political role of the corporations was at a low point.

The first chronicler to articulate an open hostility, not merely toward individual actions of the guilds and their Consuls, but to the very assumptions and foundations of corporate politics, was Marchionne di Coppo Stefani, who wrote in the aftermath of 1378. The spectacle of the independent guild of dyers making life miserable for the woolen cloth manufacturers of the *Arte della Lana* was more than Stefani could tolerate,⁴⁵ and he began to associate the whole idea of guild government, not altogether wrongly, with social and class revolution. This negative posture toward the aims of corporatism in his own day led Stefani, like Villani, to play down the significance of the guilds in the political history of Florence. They appear in his account of the distant 1260s, but not in that of the 1290s, and only sporadically in that of the 1340s.⁴⁶ Stefani not only neglected guild history; he specifically rejected the underlying principles of any corporate polity. The “popolazzo” and the “mezzani” of Florence, he asserted, exist “con niun ordine”—that is, in the absence of any established institutional framework. Here, in view of the experience of 1378, the wish was certainly father to the thought, but Stefani’s explanation for the presumed inability of the Florentine middle and lower classes to organize themselves politically is nonetheless revealing: “they are too numerous to be gathered together or to come to any firm decisions.”⁴⁷ By rejecting the possibility of meaningful consent obtained through the collective participation or representation of communities of equal members, Stefani, in effect, denied the validity of the corporate state. And on these grounds he condemned Walter of Brienne’s decision in 1343 to organize the processions for the festival of Saint John the Baptist by guilds instead of by *gonfaloni*, “with each guild separate and independent from the rest. The citizens, remembering that this offering used to be made through the *gonfaloni* and seeing the magnification of the *gente minuta* . . . , became highly indignant, because this was *beyond all human and divine reason*.”⁴⁸ Stefani’s reasoning is a direct reflection of the dilemma posed by guild government for the Florentine upper classes: to organize the procession by guilds, with each guild separate and autonomous, was, as a matter of numerical necessity, to give prominence to the middle and lower classes; and to do this, in the minds of those who had suffered through the events of 1378, was madness.

⁴⁵ Stefani, *Cronaca fiorentina di Marchionne di Coppo Stefani*, ed. N. Rodolico, in *Rerum Italicarum Scriptores*, 30, pt. 1 (Città di Castello, 1955): 386.

⁴⁶ *Ibid.*, 51, 70–74, 199–223.

⁴⁷ *Ibid.*, 194: “Il popolazzo ed ancora i mezzani, che non vivono con niun ordine, e perocchè sono troppi a ragunarsi, o ad intendersi, s’imbeccano per gli orecchi, o per esser loro toccato la spalla, e col presente lusingamento senza nullo provvedimento di futuro da loro, o d’altronde non è chi loro lo mostri, s’accordano a chi loro parla, e credenti sono.”

⁴⁸ *Ibid.*, 202–03: “La festa di S. Giovanni fece fare per arti e non per gonfaloni, e ciascuna arte per sè . . . ; Onde li cittadini, che si ricordarono della offerta co’ gonfaloni, e veggendo magnificare la gente minuta e scardassieri ed inalzargli, sdegnarono forte di ciò, perchè era fuori d’ogni umana e divina ragione. . . .”

But in rejecting the ideals and assumptions of corporatism, Stefani began to put something in their place. He praised the patrician families of Florence, “the *grandi* of wisdom, gentility, and order,” for the manner in which they made decisions among themselves: by honoring and “revering the wisest person of their line or, at the most, the wisest few.” Because there are “fewer of them to be convened and consulted,” it is easier for them to discuss their affairs and to bring into harmony “la volontà degli appetiti.”⁴⁹ Here was an alternative vision of the political process based on the related notions of decision-making authority concentrated in the hands of the experienced few and willing consensus of a wider community that “reveres” their wisdom and judgment. In essence, Stefani substituted the patrilineal family (the *legnaggio*)—with its built-in hierarchy and notions of descending, but benevolent, authority—for the fraternal corporation of equal members—in which authority ascended from the membership to its delegated leaders—as the preferred model for understanding and ordering the political world of the commune. The use of the family as a model for conceptualizing political relationships in a community dominated by aristocratic power and hierarchical values (which nonetheless felt the need to justify the existence of the ruling “one or few” with the notion that their authority served the common good and thereby rested on the implicit consent of its beneficiaries) was destined for a long and successful career in the second century of the Florentine republic. Again, it was Bruni in the *Life of Dante* who popularized the idea with Aristotelian notions of the family as the “original” and fundamental political unit, “from the multiplication of which is born the city.”⁵⁰ In this transformed world of political assumptions, it was no accident that the most powerful and revered Florentine of them all, Cosimo de’ Medici, was dubbed *pater patriae*. This mode of thought led Leon Battista Alberti, some decades later, to claim in the *De Ieriarchia* that “a city is composed of many families, so that in itself it is almost a large and fine family, and conversely the family is like a small city. . . . It is necessary to assume that the family is a body similar to a republic.”⁵¹

The rejection of corporatism and the tentative formulation of a contrasting model of consensus politics, pioneered by Stefani in the 1380s, were assimilated and elaborated into a new vision of Florentine republicanism in the early fifteenth century by Leonardo Bruni. In his hands, the guilds were written out of Florentine history, expunged from the tradition they had helped to shape.⁵²

⁴⁹ *Ibid.*, 194: “. . . li Grandi di senno, di gentilezza, d’ordine e le famiglie, che hanno sempre reverenza a uno il più savio del loro legnaggio. o a pochi, è poco accordare, e discutono le loro faccende, e veggonne il meglio; dico, de’ loro appetiti non il meglio sempre, ma la volontà degli appetiti accordano piuttosto, o bene, o male, che piglino, perchè hanno meno a consigliare ed a ragunare; ai popoli, come detto è, è impossibile.” On the *legnaggio* and its leadership, see F. W. Kent, *Household and Lineage in Renaissance Florence* (Princeton, 1977), 6–7, 245–46. Kent’s conclusion here that “formal family meetings were usually attended only by the heads of households or by influential leaders of each line” and that “a *consorteria* was led, and represented, by a group of elders or heads of households” seems to confirm the accuracy of Stefani’s view of the patterns of authority and decisionmaking in upper-class Florentine families.

⁵⁰ Thompson and Nagel, *The Three Crowns of Florence*, 61.

⁵¹ Leon Battista Alberti, *Opere Volgari*, ed. C. Grayson, 2 (Bari, 1966): 266–67.

⁵² Interestingly, Bruni’s paraphrase in the *Laudatio* of the maxim *quod omnes tangit* has nothing at all to do with the guilds or their constitutional functions. He referred to it in explanation of the role of the councils of

In the *Histories of the Florentine People*, he mentioned them only in the context of the 1378 revolution, as if to emphasize their subversive and socially disruptive tendencies. That the guilds had a significant or lasting impact on Florentine politics or society would be impossible to tell from the pages of Bruni's *Histories*.⁵³ So Bruni grounded his new republicanism in the centralized sovereignty of the state and in the consensus of a broad, unified, and undifferentiated political class. And, in fact, the number of Florentines considered and accepted for high office was enormously expanded in these years: nominations for the *Signoria* in the much smaller urban population of 1433 were nearly twice what they had been in the mid-fourteenth century;⁵⁴ the number of approved candidates, over two thousand in the scrutiny of 1433, was three times greater than in 1391.⁵⁵ This explosion of the office-holding class provided the wide base for the consensus politics of the Albizzi years. Of course, only a fraction of this class enjoyed the authority of the decision-making inner circle,⁵⁶ and the difference between a Buonaccorso Pitti, on the one hand, and a Goro Dati or a Giovanni di Paolo Morelli, on the other, is a measure of that distinction, even within the limits of the upper class.⁵⁷

Ultimately, consensus politics turned on a separation between participation and power. The legitimacy and stability of this system depended upon the consensus represented by the thousands whose names were introduced into the electoral process and who thereby gained, as Bruni so accurately observed in the funeral oration on Nanni Strozzi, the "hope" of ascending to the honors of high office. The political class of the guilds was co-opted to the purposes of the ruling group and wooed away from its traditional, but essentially always ambivalent, attachment to corporate politics. In the process the guilds were reduced to offices of the state,⁵⁸ subordinated legally and constitutionally to a sovereignty in which they no longer had any part. If the corporate ideal can be said to have survived beyond the 1380s or the 1390s, it was largely in the form of legislative obstructionism on the part of disgruntled artisans or futile

the people and the commune which had to approve all regular legislation. These councils, however, had no power of initiative. In Bruni's version, moreover, the *omnes* of *quod omnes tangit* have become the *multos*, indicating individuals instead of corporate or collegiate bodies, a small but significant change that effectively ruled out any application of the maxim like that contained in the first rubric of the Ordinances of Justice. For the text, see Baron, *From Petrarch to Leonardo Bruni*, 260; for comment, see Rubinstein, "Florentine Constitutionalism," 446.

⁵³ On the significance of Bruni's neglect of the guilds and Machiavelli's reaction to it, see my "Arti and Ordini in Machiavelli's *Istorie Fiorentine*," in Sergio Bertelli and Gloria Ramakus, eds., *Essays Presented to Myron P. Gilmore*, 1 (Florence, 1978): 161–91.

⁵⁴ According to Villani, 3346 in 1343; *Cronica*, 4: 49. By 1433 they had reached 6354; D. Kent, "The Florentine Reggimento," 587.

⁵⁵ D. Kent, "The Florentine Reggimento," 587, 613–21, 633. Kent's figures are 677 for 1391 and 2084 for 1433.

⁵⁶ For some very interesting attempts to define and identify the members of this inner circle, see D. Kent, *ibid.*, 601–08; and Brucker, *Civic World of Early Renaissance Florence*, 262–82. But one should also take account of the illuminating perspectives on this problem that emerge from Anthony Molho's study of the fiscal crisis of the 1420s and 1430s in *Florentine Public Finances in the Early Renaissance, 1400–1433* (Cambridge, Mass., 1971).

⁵⁷ Brucker, *Civic World of Early Renaissance Florence*, 261–62. The gulf between Pitti and Dati can be sampled in English translation in *Two Memoirs of Renaissance Florence: The Diaries of Buonaccorso Pitti and Gregorio Dati*, ed. Gene Brucker (New York, 1967). For Morelli, see his *Ricordi*, ed. V. Branca (Florence, 1969).

⁵⁸ In 1414 the statutes of the guilds were corrected to include the declaration that "the office of Consul [of the guilds] was and is an office of the people and of the city of Florence." Having relegated the leadership of

conspiracies hatched by still unresigned workers.⁵⁹ In the eyes of the upper classes, however, it survived only as conspiracy and sedition perpetrated by malcontents, not as a political and constitutional program grounded in the respectability of law and custom. Cut off from their political function, the guilds no longer served as vehicles through which the aspirations and grievances of social groups could find adequate, rational, and effective expression. The corporate republic had run its course.

the guilds to the position of state officials, it is ironic that the same declaration went on to reassert the old corporate formula "quod civitas Florentie regitur et gubernatur per dictas artes"; *Arte del Cambio*, 5, f. 128v. This notion had in fact been emptied of any real constitutional significance, and, if anything, the reverse was by now true. On this important reform of the guild statutes, see the analysis of Julius Kirshner, "Paolo di Castro on *Cives ex Privilegio*: A Controversy over the Legal Qualifications for Public Office in Early Fifteenth-Century Florence," in Anthony Molho and John A. Tedeschi, eds., *Renaissance Studies in Honor of Hans Baron*, (Florence, 1971), 244-45.

⁵⁹ Brucker, *Civic World of Early Renaissance Florence*, 17-18, 472-78. The one potentially serious instance of insurrection from the guild community after the early 1380s occurred in 1393, but it too was a failure; *ibid.*, 17-18, 90-94. Machiavelli also recognized it as such and turned the episode into a symbol of the overall failure of Florentine corporate politics in his *Istorie Fiorentine*; see Machiavelli, *Istorie Fiorentine*, ed. F. Gaeta (Milan, 1962), 261-63; and, on this point, see my "Arti and Ordini," 180-82.

Humanists, Scientists, and Pliny: Changing Approaches to a Classical Author

CHARLES G. NAUERT, JR.

OUT OF THE LEARNED CHAOS that followed the collapse of Jacob Burckhardt's synthesis of Renaissance civilization, certain useful guidelines for the modern student have emerged in recent years. One of the most valuable of these is that scholars ought not to present the humanists as spokesmen for a new "humanistic" philosophy of life, for humanism was really just a program of study based on ancient classical literatures and focused on a rather limited range of subjects, the *studia humanitatis*.¹ Although in the long run the humanist's studies and his underlying value system might have implications for all fields of learning, his direct concerns were rather narrow: he dealt with two of the three parts of the medieval *trivium*, grammar (embracing the study of language) and rhetoric (embracing the study of literature and the art of persuasion), and also with poetry, history, and moral philosophy. All of these subjects were approached through the reading of ancient literature. At the most basic level, then, humanists were students of ancient language and ancient literature.² The renaissance that they so firmly believed they were bringing about was a rediscovery of classical languages and classical texts.

Such an approach to Renaissance humanism makes good sense in terms of Greek cultural influence, which (with the important exception of Aristotle) was very little known in the Latin West between the fifth and fifteenth

My long-term research on Pliny for this article and for the census of Pliny commentaries that will appear in the forthcoming volume of *Catalogus translationum et commentariorum* has been supported by a number of grants from the Research Council of the University of Missouri-Columbia. A fellowship from the American Council of Learned Societies for 1975-76, although supporting a very different project, involved many of the same individuals and facilitated completion of my work. I also thank the former editor of the *Catalogus*, Paul Oskar Kristeller, and the present editor, F. Edward Cranz, for their encouragement and advice. My ancient history colleagues, Fordyce W. Mitchel and Robert J. Rowland, Jr., have helped me plug the many gaps in my knowledge of classical scholarship.

¹ This viewpoint, propounded vigorously during the past generation by Paul Oskar Kristeller, constitutes, if not the total wisdom about Renaissance humanism, at least the essential starting-point for study of that subject. See his *Renaissance Thought: The Classic, Scholastic, and Humanistic Strains* (New York, 1961), esp. 8-12, 108-11, 120-24. The masterly history of Renaissance studies by Wallace K. Ferguson abundantly documents in its closing chapters the chaos wrought by the critics of Burckhardt and thus underlines the importance of Kristeller's narrow, cautious, but immensely valuable way of approaching humanism; Ferguson, *The Renaissance in Historical Thought: Five Centuries of Interpretation* (Boston, 1948).

² L. D. Reynolds and N. G. Wilson, *Scribes and Scholars: A Guide to the Transmission of Greek and Latin Literature* (2d ed., Oxford, 1974), 108-10; and Roberto Weiss, *The Spread of Italian Humanism* (London, 1964), 12. Both authors essentially follow Kristeller's approach to humanism.

centuries, and which certainly was made accessible by the manuscript discoveries and critical editions of humanist scholars during the fifteenth and sixteenth centuries. Yet to think of Renaissance humanism as merely a process of literary rediscovery rather than as a new philosophy of life is to trade one gross oversimplification for another. Intellectual history can never be adequately written merely by tracing literary influences, for rediscoveries of literary sources are rarely, if ever, fortuitous. Societies generally rediscover the literary works that they need and commonly show indifference to all past writings that do not speak to their current interests. The relative suddenness of the recovery of ancient Greek literature masks the much deeper and much more significant question of why, after so many centuries of neglect, Western scholars were willing to put forth the great effort required to establish the study of a new language and a new literature. Part of the answer, of course, lies within the field of social history: times had changed. But attention to changing approaches to the classical inheritance can also shed light on the problem.

In applying the humanists' theme of "revival, rediscovery, rebirth" to the Latin heritage, the ambiguities of this theme become more readily apparent than in the case of the Greek. Although much of Greek literature really did have to be rediscovered, knowledge of Latin language and literature had never been lost in Western Europe: wherever literacy and organized education survived, they were Latin. There were, it is true, some dramatic rediscoveries of Latin authors during the Renaissance; but most of the works of most authors were known to educated men during the Middle Ages.³ Yet the concept of "rediscovery" or "rebirth" of this Latin heritage was a central element in the thought of the humanists and can be seen clearly even in the generation of Petrarch, when the acquisition of Greek language and literature had not begun. What really occurred—in both Greek and Latin studies—was not a simple rediscovery of lost books. Rather, a shift of values and of interests made well-known authors interesting in new ways and also made the discovery of "new" ancient authors seem worth the rigors of extended travel and the toil of learning a new language (Greek) and of reshaping an old one (Latin).

SINCE SHIFTS IN INTERESTS AND VALUES, rather than mere discovery of ancient texts, constituted the real nature of Renaissance cultural change, much can be learned from close attention to the changing appreciations of a major Roman author who never had to be rediscovered, Pliny the Elder (ca. 23–79 A.D.). His *Natural History*, the most important work of natural science by an ancient Roman author, was never lost; but it underwent several important stages of re-evaluation during the fifteenth and sixteenth centuries. Study of the changing approaches to this book allows one not only to trace the influence of a major Latin author but also to observe both Italian and non-Italian humanists at work at their real trade, the study of a classical text—a text that had

³ Kristeller, *Renaissance Thought*, 6.

always been known but that the humanists nevertheless claimed to have rescued from neglect and corruption.

Pliny's *Natural History* became an influential book of natural science from the moment of its publication. There is evidence for at least some knowledge of it during every medieval century.⁴ Already cited, epitomized, and even plagiarized by late Roman and patristic authors, this long and learned survey of ancient scientific knowledge survived the breakdown of Roman political and educational institutions. Although such a long and detailed text was difficult to transmit and reproduce in an age of manuscript books, it was too valuable as an encyclopedic summary of previous knowledge to be lost or to be replaced adequately by derivative works like the *Collectanea* of Solinus. It was known to such medieval encyclopedists as Saint Isidore of Seville, the Venerable Bede, Vincent of Beauvais, and Bartholomaeus Anglicus, for all of whom Pliny ranked as a major authority. Its surviving influence is also shown by its frequent citation (or unacknowledged use) as a source for study of natural philosophy (for example, by John of Salisbury and Albertus Magnus). Even clearer proof of its widespread diffusion and popularity is the survival of more than two hundred full or partial manuscript copies.⁵ The difficulty of transmitting such a long text in an age of handwritten books was offset in part by its topical organization, which allowed writers interested in only certain of the many subjects it contained to make do with partial copies and to dispense with the labor of copying out the whole thing. Numerous manuscripts containing excerpts from Pliny, often selected on a topical basis, indicate another way of solving the problem of transmitting the valuable scientific lore buried within Pliny's vast encyclopedia.⁶

But, although Pliny's work was attractive and readily available to medieval scholars, they used him in a way that reflected the needs and interests of their own time. Essentially, he was a storehouse of useful information and was used as an encyclopedia. There seems to have been little concern about the accuracy of the text. Medieval readers of Pliny wanted material for specific purposes—facts, anecdotes, names—and so they not only contented themselves with the available texts, but also willingly satisfied themselves with portions, or even scattered excerpts, from what must have seemed a long, diffuse, and costly text. Because Pliny's data rather than his point of view were wanted, there was little effort to expound the broader meaning of what he wrote. Medieval manuscripts of Pliny contain many glosses to explain specific words, but a careful search has so far revealed no commentaries from the medieval period. For the purposes his medieval readers had in mind, Pliny did not require extended commentary either to improve the text or to expound his meaning. Though widely read, furthermore, Pliny's work was not taught:

⁴ For more detail, documentation, and bibliography on medieval knowledge of Pliny, see my "Caius Plinius Secundus," in F. E. Cranz, ed., *Catalogus translationum et commentariorum*, vol. 4 (forthcoming), esp. the introductory section, "Fortuna."

⁵ This is the conventionally accepted number; see, for example, Wilhelm Kroll, in Pauly-Wissowa, RE 21, pt. 1 (1951): 431–34.

⁶ For references to some modern studies of these manuscripts of excerpts, see my "Caius Plinius Secundus," nn. 18–19.

Aristotle, not Pliny, provided the medieval universities with their texts for the study of natural philosophy. Hence, medieval teachers produced commentaries on Aristotle, but not on Pliny, as a natural by-product of classroom instruction.

The early Italian humanists—Petrarch, Boccaccio, Coluccio Salutati, and Niccolò Niccoli in particular—were interested in Pliny, for he was a major Roman author.⁷ In addition to the scientific information that their medieval predecessors had always drawn from him, the humanists found “new” materials that appealed to their kind of interest in classical civilization. Portions of Pliny gave them a valuable geographical description of the Roman Empire at its height. Books 34 and 35 provided a survey of ancient art. But to a group of literary and linguistic scholars like the humanists, probably the most valuable material available in the *Natural History* and neglected by the Middle Ages was literary and linguistic. Pliny drew heavily and explicitly on earlier Greek and Roman authors, many of whose works were preserved only in his quotations from them.⁸ Most valuable of all for the practicing classical scholar, he provided a classical vocabulary of scientific words not available in any other ancient Latin author. As the humanists during the fifteenth century began the task of translating the far richer body of Greek scientific works into Latin, they drew much of the necessary Latin terminology from Pliny: for example, Theodore of Gaza in his translations of Aristotle and Theophrastus.⁹ This linguistic value of Pliny was what the first great Pliny commentator, Ermolao Barbaro (1454–93) had in mind when he wrote of Pliny, “Without him, Latin scholarship could hardly exist”¹⁰—a view subsequently echoed by humanist students of the *Natural History* such as Marino Becichemo (ca. 1468–1526), the German printer Eucharius Cervicorn, and the great Spanish commentator Fernando Nuñez de Guzmán, commonly known as Pintianus (ca. 1475–1553).¹¹ Pliny was still read for his information on natural science, but the humanists had found new and characteristically humanistic uses for him.

ALTHOUGH EARLY HUMANISM showed signs of a new approach to the *Natural History*, the real innovation of the fifteenth century in the study of Pliny was probably less a result of humanism than of the new technology of printing.

⁷ Petrarch's own copy of the *Natural History*, with his marginal notes and one note in the hand of Boccaccio, has survived; see Bibliothèque Nationale, Paris, MS Lat. 6802. Notes in the hands of Salutati, Niccoli, and also Bartolomeo Platina are found in another manuscript of Pliny's: Bodleian Library, Oxford, MS Auct. T. I. 27.

⁸ The best expression of this awareness that Pliny offered access to materials from ancient books now lost appears in the dedicatory epistle by Beatus Rhenanus to his commentary *In C. Plinium* (Basel, 1526), 3: “Quis enim nescit,” etc. The Latin text of this and of subsequent quotations from Pliny commentators is also reprinted in my “Caius Plinius Secundus.” For ease in locating the quotations, I give the Latin *incipit* where that would be helpful.

⁹ Philip Melanchthon in his *Oratio de vita Rodolphi Agricolae* reported Agricola's story about Theodore's use of Pliny. See his *Opera*, ed. Carolus Gottlieb Bretschneider, in *Corpus Reformatorum*, 11 (Halle, 1843): 442.

¹⁰ Barbaro, *Castigationes Plinianae*, 2 vols. in 1 (Rome, 1492–93), 1: f. a2r.

¹¹ Becichemus, *Elegans ac docta in C. Plinium praelectio* (4th ed., Paris, 1519), f. a3r; Cervicorn, letter in Caesarius's edition of *C. Plinii Secundi Naturalis historiae opus* (octavo ed., Cologne, 1524), f. 0006r; and Fernando Nuñez de Guzmán [known as Pintianus], *Observationes* (Antwerp, 1547), f. 4v–5r.

Certainly, humanism did have an inherent tendency to develop a scientific method for the criticism, evaluation, and restoration of texts. The career of Lorenzo Valla (1405–57), whose life antedated the introduction of printing to Italy, is a good illustration of this point. Yet in many respects Valla was ahead of his generation, and the real development of a tradition of critical scholarship applying linguistic science to the restoration of ancient texts occurred only after the rise of the printing industry.¹²

In the study of Pliny, the introduction of printing was the direct cause of a major shift of outlook. Pliny's *Natural History* was printed early (the first edition in 1469 by the printer Johannes de Spira at Venice) and often (a total of fifteen incunable Latin editions, plus three incunables of the Italian translation by Cristoforo Landino, plus scores of sixteenth-century editions in Latin and in several vernaculars).¹³ Medieval readers and scribes seem not to have felt dissatisfaction with their texts of Pliny, even though, in fact, those texts were full of textual corruptions. But during the Renaissance one minor and one major new development occurred. Although still not ordinarily a schoolbook, the *Natural History* became the subject of public lectures by humanistic scholars who dealt with issues of text and interpretation not normally raised by medieval readers.¹⁴ Far more important was the printing of the text. The preparation of copy for these editions was a proper task for humanistic scholars; and, although the editor of the first edition of 1469 is unknown, the work of some identifiable humanistic editor stands behind every other Renaissance Latin edition of the work.

The responsibility of preparing copy for a printed edition of Pliny compelled the humanist to face issues that his medieval and even his humanistic predecessors had neglected. Unlike work on a private handwritten copy, editorial work on a text for printing put the editor publicly on the record. The text that he established (with all of its virtues and all of its defects) would be widely diffused and inevitably would become a standard version in a way in which no manuscript copy ever could. The editor's reputation as a scholar would be deeply affected by his peers' judgment of his work on a major author like Pliny. Hence, a new concern for establishing a correct text, removing obvious errors, and restoring corrupt passages emerged. Systematic textual restoration based on manuscripts and ancient authorities developed; and the whole field of Pliny scholarship came to be dominated by textual concerns.

¹² On the cultural significance of the introduction of printing, see Elizabeth L. Eisenstein, "Some Conjectures about the Impact of Printing on Western Society and Thought: A Preliminary Report," *Journal of Modern History*, 40 (1968): 1–56, and "The Advent of Printing and the Problem of the Renaissance," *Past & Present*, no. 45 (1969): 19–89.

¹³ Arnold C. Klebs, "Incunabula scientifica et medica: A Short Title List," *Osiris*, 4 (1938): 254–56 (items 786, 787). For the vernacular editions, see my "Caius Plinius Secundus."

¹⁴ The Florentine humanist Angelo Poliziano recorded in his own copy of the 1473 edition of Pliny that in 1490 he lectured on that author to a group of English and Portuguese students. See Bodleian Library, Oxford, MS Auct. Q. 1. 2., f. 401v: "Quin hoc ipso anno priuatim Britannis quibusdam et Lusitanis qui se Florentiam contulerant literarum studio cupientibus atque a me petentibus enarraui septimestri spatio." Several commentators on Pliny traced the origin of their commentaries to lectures in schools or to the general public: Vitellius, Sabellicus, Becichemus, Caesarius, Guglinger, Pintianus, and of course the whole "Wittenberg group." See the sections on these scholars in my "Caius Plinius Secundus."

This sort of change probably took place in the treatment of many classical authors. It certainly occurred in the case of Pliny, whose text presented (and still presents) difficult editorial problems because the manuscript tradition is confused and imperfect.

As humanist editors faced this new task of establishing and improving the text of the *Natural History* and as humanist lecturers (sometimes the same persons) faced the task of interpreting the meaning of what Pliny had written, the commentary on Pliny was born. Only one of the known commentaries antedated the invention of printing, and it is more an epitome than a true commentary. All but one of the major Renaissance commentaries (those by Barbaro, Berault, Beatus Rhenanus, Gelen, and Dalechamps) directly derived from research associated with the preparation of a critical edition; and the one exception, the commentary by Pintianus, arose out of lectures at the University of Salamanca.¹⁵ Furthermore, the discussion of Pliny by influential humanists who did not compose commentaries, but whose scholarly works affected the way he was read, also focused on critical analysis of the text: among them were Angelo Poliziano, Guillaume Budé, Adrien Turnèbe, Josephus Justus Scaliger, Konrad Gesner, and Justus Lipsius.¹⁶

The text itself was the central object of attention for these humanistic scholars. What they sought to do was to restore the integrity of the original wording,¹⁷ corrupted by centuries of what they regarded as wanton neglect of ancient literature by their own ancestors.¹⁸ Despite their disdain for medieval "barbarism," these humanistic commentators and editors seemed even more inclined to attribute the errors of previous editions to the carelessness and greed of earlier humanistic editors and, above all, of the printers. Indeed, the earliest real commentary known, that of Niccolò Perotto (composed about 1470–73), not only passed severe judgment on the competence of the first identifiable editor of a printed *Natural History*, Giovanni Andrea de' Bussi (1417–75), but also set forth in its dedicatory epistle a sweeping plan for prepublication censorship of all books so that careless printers would be prevented from flooding the world with thousands of copies of error-filled books.¹⁹ The greatest of fifteenth-century commentators, Barbaro, claimed to have corrected some five-thousand errors in the two earlier editions that he made the basis for his emendations;²⁰ and he later added more corrections and admitted that no doubt there were still other errors that he had missed. Later commentators, while almost always praising Barbaro, also claimed to have added great numbers of textual improvements, sometimes corrections

¹⁵ See the section on each of these commentators in my "Caius Plinius Secundus."

¹⁶ For some discussion of these scholars' relation to the textual tradition of Pliny, see *ibid.*, esp. the section entitled "Doubtful or Spurious Commentaries."

¹⁷ Barbaro, *Castigationes*, 1: f. 16r: "Malui tamen," etc.

¹⁸ Beatus Rhenanus, *In C. Plinium*, 3 (dedicatory epistle to Johannes à Lasco): "Saepe causas male," etc.

¹⁹ Perotto, *Commentariolus in prooemium Plinii* (n.p., n.d. [1480–82]), lacking foliation and signatures: "Solebam nuper," etc. The whole *Commentariolus* may most conveniently be found appended to many editions of Perotto's most famous work, *Cornucopiae*.

²⁰ Barbaro, *Castigationes*, 1: f. a1v: "Quinque milia," etc.

missed by him and sometimes errors subsequently introduced by careless printers.²¹ This claim to have restored a corrupt *Natural History* to its ancient integrity was common among humanistic commentators and editors. Even the editor of the first printed edition in 1469, whose identity remains a mystery, claimed in his versified colophon to offer a restored text.²²

THIS WORK OF TEXTUAL RESTORATION, conducted by professional philological experts amid constant claims to have uncovered flaws in the work of their predecessors, led inevitably to bitter personal rivalries, as Pliny scholars competed for reputation, patronage, and academic appointments.²³ But these disputes over how to correct an obviously corrupt and very difficult classical text also led to explicit and fruitful discussion of the proper canons of editorial practice. From the very first these humanists agreed on the general principle that "old codices" must be sought out and given preference over the current printed versions. Perotto, for example, emphasized the need for proper evidence rather than mere conjecture in emending Pliny,²⁴ as did Filippo Beroaldo the Elder, author of two short but influential commentaries and editor of a highly influential and oft-reprinted edition of the *Natural History*.²⁵ Barbaro regularly appealed to the "old copies"—that is, the manuscripts—against the readings found in printed editions.²⁶ The weighty authority of the commentary by Beatus Rhenanus rested on his access to an important manuscript, *codex Murbacensis*, which is still cited (at second hand, through Beatus's commentary) in the modern editions. Substantial differences of opinion arose, however, over the degree to which an editor might introduce emendations based on other authorities, such as discussions of similar topics by other classical writers. Though he urged caution in the making of textual changes,²⁷ Barbaro openly based his work not only on manuscripts but also on materials drawn from other ancient authorities and from other sections of Pliny's own book;²⁸ and Beroaldo did the same.²⁹ The commentator Marc'

²¹ For example, Johannes Caesarius in 1524 claimed to have made some four thousand additional improvements to the text. See his edition of *C. Plinii Secundi Naturalis historiae opus* (octavo ed., Cologne, 1524), 1: f. AAA2v.

²² Edition of 1469, verso of the last leaf.

²³ Thus, the second commentary of Cornelio Vitellio, *In defensionem Plinii et Domitii Calderini contra Georgium Merulam*, which was first printed in his *De dierum, mensium annorumque obseruatione* (Venice, 1481–82?), was part of a personal attack on the humanist Giorgio Merula; the short commentary by Raffaele Regio, *Epistolae Plinii, qua libri naturalis historiae Tito Vespasiano dedicantur, enarrationes* (Venice, 1490), was part of a volume designed chiefly to attack the reputation of his rival Merula; and Regio himself was the object of attack in the *Elegans ac docta in C. Plinium praelectio* (Brescia, 1503?) by Marino Becichemo, whom Regio had bested in the competition for the chair of rhetoric at Padua in 1503. These controversies have generally been dismissed as mere examples of the humanists' jealousy and spitefulness and have never been thoroughly studied. For more material on these three individuals and their Pliny commentaries, see my "Caius Plinius Secundus."

²⁴ Perotto, *Commentariolus*, no foliation or signatures: "neque me ista," etc.

²⁵ This commentary was part of Beroaldo's influential edition of Pliny (Parma, 1476). See note 29, below, for the specific passage cited.

²⁶ Barbaro, *Castigationes*, 1: f. a7r: "In omnibus," etc.

²⁷ *Ibid.*, 2: f. 6bv (author's postscript to all parts of the *Castigationes*): "In ipso laboris," etc.

²⁸ *Ibid.*, 1: f. 15v: "Ea nos graecis," etc.

²⁹ Beroaldo, commentary prefixed to his edition of Pliny (Parma, 1476), f. 351v: "Quod ferme uideor," etc.

Antonio Coccio, commonly known as Sabellicus, even proposed emendations having no ancient authority, based on conjecture; but, in a slightly later work on Pliny, Sabellicus concluded that such conjectural emendations, lacking the authority of a manuscript or another ancient writer, should only be suggested in the marginal notes, not introduced into the text itself.³⁰ The influential French commentator Nicolas Berault (1473–1550) was willing to accept conjectural readings based not only on stylistic considerations but also on the editor's general knowledge about the subject under discussion.³¹ A similar willingness to emend Pliny on the basis not just of old manuscripts but also of parallel passages in other ancient books and even of surviving ancient statues and inscriptions was stated by Angelo Poliziano, who never wrote a commentary on Pliny but whose *Miscellaneorum centuria prima*—held in great authority among humanistic scholars—dealt with several sections of the text.³² The German editor and commentator Johannes Caesarius, whose work on Pliny also had great authority in its own time, emended the text arbitrarily, using as his source similar passages in the works of Aristotle, Plato, Cicero, and other ancient writers.³³

In general, however, the humanists whose work had lasting influence on the text of Pliny were cautious about conjectural emendations based on *auctores* (other ancient writers who dealt with the same subjects) rather than on *codices*. Beatus Rhenanus, though warmly appreciative of Barbaro's achievements, regarded manuscripts of the ancient author himself as the only acceptable authority and openly criticized Barbaro for making conjectural changes based on other writers such as Aristotle.³⁴ Beatus stated this opinion even more clearly in his attack on the commentary by the French nobleman Étienne de l'Aigue (Stephanus Aquaeus), even rejecting Aquaeus's use of an ancient book closely related to the *Natural History*—the *Collectanea* of Solinus—as an authority for emending Pliny. He argued that editors must avoid arbitrary textual changes, even if they seem to improve the sense of a difficult passage. The editor must accept what the *codices* show the ancient author to have written, whether or not it made sense.³⁵ Still later, in his dedicatory epistle for an edition (1537) of the Pliny commentary by Francesco Massaria of Venice, Beatus criticized both Barbaro and his own friend Sigismund Gelen for their

³⁰ See, for example, Sabellicus, *Emendationes seu annotationes in Plinium* (original edition, with title *Observationes linguae Latinae*, was undated, probably Venice, ca. 1497), in his *Annotationes veteres et recentes* (Venice, 1502), f. a2v, esp. f. b2v: "Haec Pliniana habet lectio," etc. He was more cautious in his second set of notes, *Observationes . . . ex varia auctorum lectione*, first printed in his *Annotationes veteres et recentes* of 1502. See this text, as reprinted by Jodocus Badius Ascensius, *Annotationes doctorum virorum* (Paris, 1511), f. 52v: "satis credo tibi," etc.

³¹ Commentary compiled by Berault (Nicolaus Beraldus) and printed with the edition of Pliny that he both edited and published (Paris, 1516), f. +ii v: "Quaedam et nos," etc.

³² Poliziano, *Opera* (Basel, 1553), 213–311, esp. 253, 264–66, 271. For his acknowledgment of the help of Lorenzo in providing access to *codices* of Pliny, other books, statues, and inscriptions, see his dedication to Lorenzo de' Medici, *ibid.*, 216.

³³ See his edition, *C. Plini Secundi Naturalis historiae opus* (octavo ed., Cologne, 1524), 1: f. 2r, and his separate commentary on Book 9, printed in Oppian, *Alieuticon* (Strasbourg, 1534), f. 66r.

³⁴ Beatus Rhenanus, *In C. Plinium*, 4: "Nam hic certissimus," etc.

³⁵ Appendix to his *Rerum germanicarum libri tres* (Basel, 1531), 188–89: "Porro hoc sunt admonendi," etc., and "Expostulet igitur," etc.

neglect of *codices* and their willingness to base emendations on passages found in other ancient authors.³⁶ Though criticized by Beatus, Gelen himself, the little-known editor whose profound learning formed the basis for much of the deservedly high reputation of classical editions by the Froben press of Basel, provided the most thorough discussion of editorial standards and of the overriding importance of manuscripts. He urged restraint in making emendations not based on manuscripts and discussed at some length the procedure to use in employing manuscripts in editorial work; he concluded that it was better to leave portions of the text unclear than to introduce arbitrary though plausible changes not based on the manuscripts.³⁷ The great Spanish humanist Pintianus also emphasized the danger of departing from the wording found in the manuscripts. Indeed, he completely omitted Book 37 of the *Natural History* from his commentary, because neither of the two old *codices* on which he based his work contained it.³⁸ The French editor and commentator Jacques Dalechamps (1513–88) also laid great stress on the six manuscripts, two of them quite old and excellent, that formed the authority for the purely textual aspect of his work, although he was willing to introduce other emendations based on scientific investigation rather than on literary conjecture.³⁹

All of this scholarly work on Pliny was humanistic in a very specific sense of the term: the humanists were dealing with the *Natural History* as a classical text, applying their knowledge of manuscripts—and perhaps of classical Latin grammar, ancient science, and other ancient authors—to the improvement of that text. That the book dealt with natural science was, in some respects, almost incidental. The same general editorial approach would have seemed applicable to any other Roman author. The dominant scholarly interest in studying Pliny was textual. It is true that by the late fifteenth century the *Natural History* sometimes also became a schoolbook. Indeed, at the University of Wittenberg, as part of the early Protestant attempt to reform the curriculum and to eliminate the authority of Aristotle, Philip Melanchthon even used Pliny instead of Aristotle as the text for his lectures on natural philosophy. A whole line of his disciples taught Pliny's work at Wittenberg and produced commentaries as by-products of their lectures.⁴⁰ But these "Wittenberg" commentaries, though interesting as an episode in the history of antischolastic curricular reform, were of small intrinsic importance. By mid-sixteenth century the inadequacy of Pliny as a textbook had become obvious, and Me-

³⁶ Beatus Rhenanus, dedicatory epistle in *Francisci Massarii Veneti in nonum Plinii de naturali historia librum castigationes et annotationes* (Basel, 1537), f. AA2v: "Nam ille nunquam," etc.

³⁷ Gelen, Dedicatory Epistle to Damiano a Goes, in his edition of Pliny (Basel, 1535), f. aaa1r: "Nos non nostra," etc.

³⁸ Pintianus, *Observationes*, f. 5r (first dedicatory epistle to Bishop Francisco Bobadilla).

³⁹ See page 85, below.

⁴⁰ This "Protestant" use of Pliny was noted in passing by George Sarton and was developed more fully but still only briefly by Karl H. Dannenfeldt; Sarton, *Six Wings: Men of Science in the Renaissance* (Bloomington, Ind., 1957), 136; and Dannenfeldt, "Wittenberg Botanists during the Sixteenth Century," in Lawrence P. Buck and Jonathan W. Zophy, eds., *The Social History of the Reformation* (Columbus, Ohio, 1972), 223–26. Also see the discussion in the introductory "Fortuna" of my "Caius Plinius Secundus," as well as the discussion there of each of the "Wittenberg group" of commentators: Melanchthon (1527), Jakob Milich (1534), Paul Eber (1556–57), and Bartholomaeus Schonborn (1557).

lanchthon himself had long since begun the reintroduction of Aristotle into the university curriculum.⁴¹

AS A WORK DEALING WITH NATURAL SCIENCE, the *Natural History* did, nevertheless, create problems not met by editors and readers of ancient books of poetry, oratory, or history. An unavoidable difficulty facing all readers who wished to acquire useful scientific knowledge from Pliny (or from any other ancient scientific writer) was the problem of terminology: how to relate the Latin terms in Pliny to the Greek and Latin terms in other works of natural science as well as to the real objects in nature to which those terms applied. Though working as a humanist textual scholar, Barbaro sensed this problem and appended to his two-part commentary a set of *Glossemata*, a glossary of the principal terms used by the author. This same problem was evident to the first non-Italian commentator, Robert Duval (Robertus de Valle), who in 1500 published not only a commentary of the usual sort but also an alphabetical glossary of Pliny's terminology, *Explanatio locorum Plinii difficiliorum*. That Pliny was still read for practical applications of the scientific knowledge he provided is also suggested by the publication of the first detailed subject-index to the *Natural History*, the *Index Plinianus* of the Viennese Franciscan Johannes Camers.⁴²

Modern historians of science, looking (often disdainfully) at the bitter and wordy quarrels among humanistic students of Pliny, have sometimes paused to reflect upon one particular controversy that seems to involve attitudes toward scientific learning rather than just personal rivalries or disputes over arcane points of philology. This debate began in 1492 when a veteran professor of medicine at the University of Ferrara, Niccolò Leonicensi (1428–1524), published a short treatise, *De Plinii et plurium aliorum medicorum in medicina erroribus*, which pointed out errors in the medical portions of Pliny as well as in the works of “barbarian” (that is, medieval Arab) physicians. Whereas Barbaro had firmly stated that the thousands of errors in the *Natural History* were due to the ignorance and carelessness of earlier copyists, editors, and printers,⁴³ Leonicensi insisted that many of the errors were the fault of Pliny himself. Leonicensi's book was almost immediately rebutted by the tract *Pliniana defensio adversus Nicolai Leonicensi accusationem* (Ferrara, 1493), written by the jurist and humanist Pandolfo Collenuccio. In subsequent years, several other figures of secondary rank were drawn into the fight, while Leonicensi held his ground and added to later editions of his work yet other short tracts demonstrating Pliny's errors, chiefly in scientific terminology.

The major older authority on this controversy, Lynn Thorndike, did not know quite what to make of the whole affair. Although he found an attack on a standard ancient authority appealing to his “Whiggish” attitude toward the

⁴¹ Peter Petersen, *Geschichte der aristotelischen Philosophie im protestantischen Deutschland* (Leipzig, 1921), 51–55.

⁴² Camers, *Index Plinianus* (Vienna: Hieronymus Vietor and Johannes Singrenius, printers; Leonardus and Lucas Alantsee, publishers, 1514). This index was appended to many later editions of Pliny.

⁴³ Barbaro, *Castigationes*, I: f. a1 v: “Quinque milia,” etc.

history of science, he felt disappointed by this “textual, rather than scientific or rational, criticism of the *Natural History*.”⁴⁴ Although both controversialists failed to live up to his ideal of scientific criticism, Thorndike was most offended by the manner of argumentation used by Leonicensio, who criticized Pliny mainly for not understanding the more ancient and more authoritative scientist Dioscorides; “as well among the Greeks as among the Latins and even the barbarians, Dioscorides was [acknowledged as] the highest and principal authority.”⁴⁵ Although neither author adopted the proper modern scientific attitude, Thorndike concluded that “Collenuccius, although a jurist by profession, displays both a wider knowledge of the *Natural History* itself and more real acquaintance with plants and nature than does Pliny’s critic, Leonicensio, who was a physician.”⁴⁶

More recently, however, two historians of botany, working independently, have returned to the Leonicensio-Collenuccio quarrel and have suggested that the incident, though minor in itself, reflects a significant shift in attitude toward ancient scientific texts on the part of scholars during the Renaissance, who may have been trained as humanists but whose approach to an author like Pliny was dominated by the utilitarian requirements of medical practice or scientific investigation. Despite his philological technique and his criticism of Pliny mainly on the grounds that he disagreed with Dioscorides, Leonicensio was important, because, by raising the issue of confusion in botanical terminology, he faced a serious practical problem for contemporary physicians and pharmacists—the use of the same term for different substances included in prescriptions (and also the use of several different terms in various texts for what was really the same drug). This problem made the selection of the proper remedy uncertain and even made prescriptions potentially dangerous to the patient.⁴⁷ Leonicensio’s emphasis on the need for accurate terminology and his contention that Pliny had introduced confusion in the nomenclature of certain drugs that were also described by the Greek pharmacological writer Dioscorides were not mere humanistic antiquarian quibbles but stood as a warning to those who gave or took medicines prescribed on the authority of ancient texts. Although he sought his answers in the books of Dioscorides, Galen, and Paul of Aegina rather than in original experimentation, Leonicensio had an outlook as a medical writer fundamentally different from that of the “mere” humanists who criticized him, such as Collenuccio or Barbaro: he

⁴⁴ Lynn Thorndike, *A History of Magic and Experimental Science*, 8 vols. (New York, 1923–58), 4: 596.

⁴⁵ Leonicensio, *De Plinii . . . erroribus* (Ferrara, 1509), f. 3v–4r. Indeed, Leonicensio claims that Dioscorides was the major source of Pliny’s botanical knowledge; *ibid.*, f. 4r.

⁴⁶ Thorndike, *A History of Magic and Experimental Science*, 4: 598. For older works on the subject, see *ibid.*, 594. For another modern but now dated account of the controversy, also rather sympathetic to the defense of Pliny by Collenuccio, see Arturo Castiglioni, “The School of Ferrara and the Controversy on Pliny,” in E. Ashworth Underwood, ed., *Science, Medicine and History: Essays on the Evolution of Scientific Thought and Medical Practice Written in Honour of Charles Singer*, 1 (London, 1953): 269–79.

⁴⁷ Peter Dilg, “Die botanische Kommentarliteratur Italiens um 1500 und ihr Einfluss auf Deutschland,” in August Buck and Otto Herding, eds., *Der Kommentar in der Renaissance*, Kommission für Humanismusforschung, no. 1 (Bonn, 1976), 225–52; and Karen Meier Reeds, “Renaissance Humanism and Botany,” *Annals of Science*, 33 (1976): 519–42. On the confusion introduced into medical practice by the pretentious erudition of late medieval and Renaissance medical scholars, see the provocative views of John M. Riddle, “Theory and Practice in Medieval Medicine,” *Viator: Medieval and Renaissance Studies*, 5 (1974): 157–84.

was primarily interested not in restoring the text of Pliny but in discovering the truth about the substances used by physicians and their patients.⁴⁸ He was explicit about the important difference between interpreting works of natural science and interpreting literary works, and he was also blunt in his insistence that factual accuracy was necessary because “the health and the life of men depend on it.”⁴⁹

Although later Renaissance medical and scientific writers continued to base their works on ancient authorities, this tendency to shift the focus of investigation from the text of an author to the need to determine the truth about nature found other expressions. Indeed, one of the German botanists whose works demonstrate this shift of emphasis, Euricius Cordus (1486–1535), had been a pupil of Leonicensio and of Leonicensio’s successor Giovanni Manardo at the University of Ferrara. His *Botanologicon* reflects Leonicensio’s new attitude.⁵⁰

THIS DISTINCTIVE NEW ATTITUDE toward the use of ancient scientific texts found one of its earliest expressions not only in Leonicensio’s attack on Pliny’s authority but also in some of the commentaries on Pliny’s work. Most humanistic commentators stuck to their *codices* and (if their editorial theory allowed) their *auctores*, behaving as purely literary restorers of the correct ancient text. But a few did not.⁵¹ All of the more independent-minded commentators were deeply involved in the study of natural science; thus, the text of Pliny, though still important to them, was only a means to their real end, the discovery of the truth about nature. After Leonicensio, the earliest student of the *Natural History* to have this attitude was the German humanist, astronomer, and geographer Jakob Ziegler. His commentary, *In C. Plinii de naturali historia librum secundum commentarius* (Basel, 1531; reprint ed., Cologne, 1550) dealt with Book 2 of Pliny, though it also contained notes on other scattered parts of the work and related short astronomical treatises by Ziegler. Although educated at Ingolstadt and Vienna, Ziegler spent a long period

⁴⁸ Dilg, “Die botanische Kommentarliteratur,” 236–39.

⁴⁹ Leonicensio, *De Plinii . . . erroribus* (ed. 1509), f. 21v: “Nam quum hic,” etc.

⁵⁰ Dilg, “Die botanische Kommentarliteratur,” 249–52. Other botanists cited by Dilg as sharing the new orientation toward determination of the truth about nature (even though still chiefly from books) rather than purification of ancient scientific texts include Konrad Gesner, Leonhart Fuchs, Otto Brunfels, and Euricius’ son Valerius Cordus. Dilg has argued that the emphasis had shifted from words to things, so that the botanical commentary had a new purpose—practical application in medicine—even though the outward form remained, as with the nonmedical humanists, the clarification of terminology; *ibid.*, 252. Among German botanists, botany was being transformed from *Geisteswissenschaft* to *reale Naturwissenschaft*. Leonicensio’s emphasis on the authority of Dioscorides’ *De materia medica* was a step in the right direction: Dioscorides was the most practically useful of all of the ancient botanists for medicine, and his book became “the most important textbook of botany in the sixteenth century”; Reeds, “Renaissance Humanism and Botany,” 537. Reeds has discerned “a marked change in the way botanists responded to the classics,” but she has concluded that they more easily combined their own experimental discoveries with continued use of ancient authorities than was possible for scholars in the field of anatomy; *ibid.*, 540.

⁵¹ Even a traditional humanist like Beatus Rhenanus knew that in theory scientific texts rested on observation of the world, and in his preface to the edition of the Pliny commentary by Massaria based his praise of Massaria’s commentary on the commentator’s extensive travels, especially in the Near East, so that his work rested on personal experience and not just on book-learning. Even so, Beatus firmly restated his general principle that commentators must stick to the authority of the manuscripts, as in fact Massaria had done. Beatus Rhenanus, Preface to *Francisci Massarii Veneti in nonum Plinii* (1537), f. AA2.

(probably 1521–31) in Italy. Indeed, he had originally been invited to Italy by Cardinal Ippolito d'Este, who at the urging of their mutual friend, the humanist Celio Calcagnini, tried to persuade him to accept the chair of mathematics at the University of Ferrara. Although Ziegler did not accept this call and went to Italy and settled in Rome only on the invitation of Pope Leo X, he maintained friendships at Ferrara and spent most of his last three Italian years there with Calcagnini. Ziegler did not cite Leonicensino, but it may be significant that he lived in the city where Pliny's critic Leonicensino (until 1524) and then Leonicensino's pupil, Giovanni Manardo, taught on the medical faculty. In the dedicatory epistle to his commentary, Ziegler explained that Pliny was difficult to understand partly because the text was defective; "but in addition the most serious cause of the difficulty is that those who have undertaken to understand Pliny were concerned with knowing and interpreting other types of subjects: humane letters, grammarians, poets, orators, historians, even philosophers. But they have studied astronomy [only] in passing, from popular authors. . . ."⁵² To Ziegler, Pliny could not be adequately understood if his work was approached only as a literary text. It had to be understood as a work of natural philosophy and interpreted by persons competent in that field of study. As one would expect, Ziegler's commentary was less concerned with emending the text of the *Natural History* than with explaining its meaning in a mathematical and astronomical sense. Another commentary printed with Ziegler's, the *Scholia in secundum Plinii*, written by Georg Tannstetter von Thannau (commonly known as Collimitius) and edited by the Swiss humanist Joachim von Watt (Vadianus), did not make an explicit distinction between the literary-humanistic approach and the mathematical-astronomical approach. But Collimitius, like Ziegler, concentrated on the scientific knowledge in the book rather than on the words of the text. Collimitius, also a graduate of Ingolstadt, where he was a pupil of Conrad Celtis, spent most of his career as a professor in the arts and medical faculties at Vienna. His work on Pliny is explicitly linked to his position as heir to the Vienna school of astronomy and cites the works of such earlier masters as Regiomontanus and Georg Peurbach.⁵³

Later in the sixteenth century, another German scientist, this time a botanist, Melchior Guilandinus, in commenting upon Pliny's discussion of papyrus in Book 13 of the *Natural History*, placed an even clearer emphasis on the need to use a scientific rather than a literary approach to the text. Although poems and most other types of literary work, he wrote, should be emended only on the authority of old manuscripts, this rule is not applicable to dealing with writers on natural history. Since the purpose of such books was to present an accurate description of things as they are found in nature, those books may be emended from other sources of information—that is, from other authoritative works on the same topic.⁵⁴ Guilandinus, in other words, ac-

⁵² Ziegler, *In C. Plinii de naturali historia librum secundum commentarius* (Basel, 1531), f. a4r: "Sed fuit postrema," etc. For bibliography on Ziegler, see my "Caius Plinius Secundus."

⁵³ Collimitius, *Scholia*, printed as an appendix to Ziegler's commentary, 443.

⁵⁴ Guilandinus, *Papyrus, hoc est, Commentarius in tria C. Plinii de papyro capita* (Venice, 1572), f. *4v–**1r: "Fateor quidem," etc.

cepted the practice of those humanist editors who emended a text on the basis of *auctores* as well as on the basis of *codices*. But this acceptance covered only those works for which the information contained, rather than the question of what the author wrote, was of paramount importance—that is, books of natural science. This commentator was an active natural scientist: author of several books on botany, friend of the famous physician Gabriele Falloppio, director of the botanical garden at Padua, and, from 1574 to his death in 1589, professor of botany at Padua.

The last great Pliny commentator of the sixteenth century, the French physician Jacques Dalechamps (1513–88), agreed with Guilandinus on the importance of manuscript authority for the text. Indeed, readings from manuscripts now lost and preserved only in his commentary are still found in the textual apparatus of modern editions of the *Natural History*. Yet he insisted in his preface that a commentary on Pliny must strive primarily to elucidate the scientific content of the text and only secondarily to fulfill the linguistic and rhetorical goals that dominated the work of nonscientific humanist editors of Pliny: “My mind had a natural proclivity to give precedence to those things that contribute to the understanding of the material rather than to those that are investigated and determined concerning the beauty and eloquence of speech, because I think that an understanding of the inner meaning of things is more useful for a wise man than vigor of expression and eloquent beauty.”⁵⁵ This explicit rejection of the humanistic ideal of eloquence is very striking, even though the author has words of praise for earlier humanistic commentators like Barbaro, Pintianus, and Gelen. Dalechamps’s emphasis on scientific content rather than literary elegance is probably all the more explicit because his work on the commentary was exactly contemporaneous with work on his important, two-volume, botanical study, *Historia plantarum* (Lyons, 1586–87). A humanistically trained scientist of the late Renaissance such as Dalechamps had come a substantial distance, intellectually speaking, from the literary and textual focus of the nonscientist editors and commentators on Pliny.

THE OUTLOOK FOUND IN Leonicensio, Ziegler, Guilandinus, and Dalechamps confirms the thesis of Peter Dilg that a significant shift in attitudes toward ancient scientific texts was under way. It was hardly a scientific revolution: questions of natural science were still treated—even among scientists—chiefly on the basis of what the ancient texts contained; and appeals to personal observation against the authority of all ancient sources were still a rarity. But for students of Pliny who stood in this newer tradition, the authority of the classical text had become different and, in reality (though not consciously), had become less. The true goal of the natural philosopher was coming to be more clearly conceived as the determination of the truth about nature, not just the determination of what a long-dead author had really written.

⁵⁵ Dalechamps, Preface to his edition of Pliny with commentary (Lyons, 1587), f. *3r: “ea mihi fuit ingenita,” etc.

Machiavelli, Guicciardini, and the Tradition of Vernacular Historiography in Florence

MARK PHILLIPS

TO UNDERSTAND EVENTS AND REPRESENT THEM MEMORABLY are the twin tasks of the historian. But analysis and representation are not easy to combine, and historians who excel at one often slight the other. In Florence before Machiavelli and Guicciardini there were two styles of historical writing, classical and vernacular, each with its own strengths. Inspired by classical example, the humanist historians concentrated on affairs of state and composed linear narratives that were clarified and augmented by reference to broad moral and political lessons. Vernacular chroniclers, on the other hand, wrote to preserve the memory of notable events, both political and nonpolitical, especially those they had themselves witnessed. Niccolò [Machiavelli] and Francesco Guicciardini drew strength from both of these styles and in different ways each made an effective combination of clarity and concreteness.

This division between classical and vernacular history was not new, for the basis of the two styles had been set long before. As Erich Auerbach has written, classical Latin prose “is an almost excessively organizing language in which the material and sensory side of the facts is rather viewed and ordered from above than vividly presented in its materiality and sensoriness.” In contrast, the proto-vernacular written by the early medieval historian Gregory of Tours is a language that “organizes badly or not at all. But it lives on the concrete side of events, it speaks with and in the people who figure in them.”¹ Gregory’s aim was to make things “vividly visible,” and, unlike classical historians, he was drawn to represent a scene simply because of its vividness. Auerbach did not deny that Gregory’s clumsy and unclassical language represents a decline, but he has forcefully argued that it is also much more. It is a “reawakening of the directly sensible” and lies on the path toward the modern sense of literary realism.²

With the Renaissance revival of classicism, these two styles of histori-

I wish to express my thanks to the Villa I Tatti and the Canada Council for their support. I am also deeply indebted to Felix Gilbert for his generosity in giving both criticism and encouragement. I hope, too, that William Bouwsma will find here a reflection of his stimulating influence as a teacher.

¹ Auerbach, *Mimesis: The Representation of Reality in Western Literature*, trans. Willard Trask (Princeton, 1953), 89–90.

² *Ibid.*, 93.

ography, classical and vernacular, came to coexist and compete within a single society. But Auerbach's sense that the vernacular contains within it something essential to modern literary realism has not been echoed in the many recent studies of Florentine historiography.³ Instead, scholars have emphasized the classical inheritance and have given less attention to the contribution of the vernacular chroniclers to the ultimate shape of Florentine historical thought. This focus on the humanist historians is especially marked in studies concerned with the fifteenth century, when the prestige of the classical models promoted by the humanists is thought to have become overwhelming.⁴ By extension, fifteenth-century humanism inevitably has become the background against which the sixteenth-century historians are viewed. Thus, it has been assumed that Machiavelli and Guicciardini wrote within a thoroughly classicized tradition. Certainly, they themselves were not humanists, but they have commonly been measured against humanist prescription or practice, and, where they diverge, they can be thought of as conscious critics of the humanist model.⁵

³ Space does not permit a full bibliography, but some of the important recent studies include the following: Felix Gilbert, *Machiavelli and Guicciardini* (Princeton, 1965), and *History: Choice and Commitment* (Cambridge, Mass., 1977); Hans Baron, *The Crisis of the Early Italian Renaissance: Civic Humanism and Republican Liberty in an Age of Classicism and Tyranny* (2d ed., Princeton, 1966); B. Ullman, "Leonardo Bruni and Humanist Historiography," *Medievalia et Humanistica*, 4 (1946): 45-61; E. Garin, *Medioevo et Rinascimento* (Bari, 1961), and "I Cancellieri umanisti della repubblica fiorentina da Coluccio Salutati a Bartolomeo Scala," *Rivista storica italiana*, 71 (1959): 185-208; Myron Gilmore, *Humanists and Jurists* (Cambridge, Mass., 1963); Donald Wilcox, *The Development of Florentine Humanist Historiography in the 15th Century* (Cambridge, Mass., 1969); Claudio Varese, *Storia e politica nella prosa del Quattrocento* (Turin, 1961); Christian Bec, *Les marchands écrivains: Affaires et humanisme à Florence* (Paris, 1967); Lonis Green, *Chronicle into History* (Cambridge, 1972); William J. Bouwsma, "Three Types of Historiography in Post-Renaissance Italy," *History and Theory*, 4 (1965): 303-14; Nancy Struener, *The Language of History in the Renaissance* (Princeton, 1970); Marvin Becker, "Towards a Renaissance Historiography in Florence," in A. Molho and D. Tedeschi, eds., *Renaissance Studies in Honor of Hans Baron* (DeKalb, Ill., 1971), 265-82; Mark Phillips, *Francesco Guicciardini: The Historian's Craft* (Manchester, 1977); Peter Bondanella, *Machiavelli and the Art of Renaissance History* (Detroit, 1973); E. Lugnani Scarano, *Guicciardini e la crisi del Rinascimento* (Bari, 1973); and J. G. A. Pocock, *The Machiavellian Moment* (Princeton, 1975).

⁴ A significant exception to this position, however, is Claudio Varese's important study of fifteenth-century vernacular writers. Varese has argued that these writers had a value and identity in many ways different from both their trecento vernacular predecessors and their humanist contemporaries. Likewise, he has stated, though without attempting to demonstrate, that they lie in the background to Machiavelli and Guicciardini. Since he concentrated heavily on the first half of the century, he has left open the question of the survival and continuity of vernacular impulses. See Varese, *Storia e politica*.

⁵ This is the effect of both Gilbert's *Machiavelli and Guicciardini* and Pocock's *Machiavellian Moment*, to name only two of the most distinguished studies. Pocock, taking the idea of civic humanism as his central model, has evaluated Machiavelli and Guicciardini against a background of Aristotelian ideas on citizenship and Polybian ideas on history. Similarly, Gilbert has outlined humanist historical theory as a background against which the new pragmatic historiography can be seen developing. Gilbert has recognized the existence of vernacular elements in the Florentine tradition, but, I think it is fair to say, has seen humanism as the creative force. An earlier statement of such views, which may be quoted both for its succinctness and the authority of its author, comes from Delio Cantimori: "This political preoccupation of humanists culminates in the work of Machiavelli and Guicciardini, who, however, by their very greatness and the precision of their thought, rise superior to the humanism in which they are rooted"; Cantimori, "Rhetoric and Politics in Italian Humanism," *Journal of the Warburg Institute*, 1 (1937): 83. Similarly, another distinguished scholar, Cecil Grayson, has written, "Some of these fields were being opened up to the vernacular in his [Bembo's] own day, especially history and political theory, where Machiavelli and Guicciardini, bred as much in humanistic historiography as in the practical experience of modern affairs, applied the lessons of antiquity to the present, and created their own vernacular instrument of expression more from usage than from precedent"; Grayson, *A Renaissance Controversy: Latin or Italian* (Oxford, 1960), 23. Clearly, Grayson has assumed that vernacular historiography had no precedents of its own and was simply the reflection of current "usage."

In short, though no single study has encompassed the whole course of Florentine historiography, there appears to be some agreement that it evolved in three progressive stages: vernacular or preclassical in the fourteenth century, humanist or classical in the fifteenth, pragmatic or posthumanist in the sixteenth. Thus, whether studying the chroniclers of the fourteenth century or the historians of the sixteenth, students of this period have taken fifteenth-century classicism as their point of reference.⁶

The humanists themselves were, of course, the first to realize their own importance. Before Leonardo Bruni, they agreed, Florentine history was largely unknown. While drawing upon the chronicles for their own narratives, they thoroughly disparaged the efforts of their *volgare* predecessors. From their own point of view this negative judgment made perfect sense, since it was built on a contrast between "true history," a form of literary art, and chronicle, a mere assemblage of the record. Ironically, however, the humanists' preference for the literary against the factual continues to prejudice the views of modern scholars, whose normal bias is just the opposite. By uncritically accepting the humanists' view of the status of the vernacular chroniclers, we narrow the limits of the Florentine historiographical tradition and impoverish the inheritance of both Machiavelli and Guicciardini.

The richness and diversity of Florentine historical writing over many generations was, I believe, essential to the maturity the city's two great historians achieved. The works of Machiavelli and Guicciardini retain important features of the vernacular style; and this unexpected survival points, in turn, to an unbroken tradition of vernacular narratives in the fifteenth century. In sum, vernacular historiography did not disappear with the quattrocento classical revival but remained a vital force linking the origins of Florentine historical thought in the fourteenth century to its maturest expressions in the sixteenth.

LET US BEGIN WITH MACHIAVELLI and with a well-known statement in the "Proemio" to his *Florentine History*. In this preface Machiavelli justified his decision to write the history of the city from its origins, which required him to go back over ground already covered by his humanist predecessors, Bruni and Poggio. His first intention, he wrote, had been to continue their narratives and imitate their procedures. But he had found these historians deficient in one crucial respect: though excellent historians of Florence's external wars, they had omitted the history of its civil discords. No subject, Machiavelli declared, is more consequential to those who rule republics than the causes and results of such discords, to which Florence in particular was especially prone.⁷

⁶ A concise indication of this is given by the titles of two recent studies: Green's *Chronicle into History* and Becker's "Towards a Renaissance Historiography in Florence."

⁷ "Perché io mi pensava che messer Lionardo d'Arezzo e messer Poggio, duoi eccellentissimi istorici, avessero narrate particolarmente tutte le cose che da quel tempo indietro erano seguite. Ma avendo io dipoi diligentemente letto gli scritti loro . . . ho trovato come nella descrizione delle guerre fatte dai Fiorentini con i principi e popoli forestieri sono stati diligentissimi, ma delle civili discordie e delle intrinseche inimicizie

Machiavelli's critical remarks about his humanist forerunners were at least an acknowledgement of his dependence on them. Although he did not acknowledge any debt to the vernacular chroniclers, he was compelled to go back to their works, since their interest in faction was as keen as his own. But did he use the chronicles merely as sources of information—as Bruni had done—or did he find some kinship with Florence's first historians? The most direct method of testing the latter possibility is to look at a prominent factional episode in Machiavelli's *History* in the light of several earlier accounts, both vernacular and humanist. The revolt in 1343 against the tyranny of Walter of Brienne, the duke of Athens, is an obvious choice; and the transformations of this famous moment in Florentine history can be traced through the successive narratives of Giovanni Villani, Marchionne di Coppo Stefani, Leonardo Bruni, and finally Machiavelli himself.

The primary account of the tyranny of the duke of Athens over the Florentines appears near the end of Giovanni Villani's *Chronicle*, and it is the most fully developed episode in that long work. All subsequent retellings of this story, including Bruni's and Machiavelli's, are based on Villani. He was a spectator of the events of 1343, and his story carries all the immediacy of the eyewitness. Nothing in the story is abstract. Streets, squares, bridges, families, all are individually named, for the chronicler assumes in his reader an equal intimacy with the city. But Villani was more than an eyewitness and his chronicle is not a simple memoir or diary. His vividness was married to an overwhelming moral seriousness. For him the history of the duke's tyranny in Florence was a moral drama. He knew without a doubt that those who had sinned would be punished and that those who were punished had sinned.⁸

Modern readers of Villani must accept moral values where they are accustomed to seeing political ones. Lacking a political vocabulary, Villani had no political category with which to characterize the uprising against the duke. His thought was for the community, not the state, and his moment of pride came when the community itself united in its own defense. Anything that detracted from that unity was, by definition, bad. He gloried in the banners and symbols of the commune and never questioned its right to take direct action to regain liberty. Thus, when the struggle against the duke reached a savage climax, Villani's vivid account makes us fully conscious of its brutality without mitigating in any way his sense that this savage revenge was the well-earned reward of sin:

In the end the *popolo* refused any pact unless the duke gave them the *conservadore*, his son, and Messer Cerrettieri Visdonini to do justice to them. To this the duke would not agree. But the Burgundians, who were besieged in the palace, banded together

... averne una parte al tutto taciuta e quell'altra in modo brevemente descritta che ai leggenti non puote arrecare utile o piacere alcuno. . . . se niuna cosa diletta o insegna nella istoria, è quella che particolarmente si describe; se niuna lezione è utile a' cittadini che governono le repubbliche, è quella che dimostra le cagione degli odi e delle divisioni della città. . . ." Machiavelli, *Istorie fiorentine*, "Proemio," in his *Opere*, ed. Flora and Cordis (Milan, 1967).

⁸ On Villani's providential view of history, see Green, *Chronicle into History*.

and told the duke that rather than die of hunger and in torment, they would hand the duke himself over to the *popolo*, as well as the said three. . . . The duke, seeing himself in such straits, consented. On Friday, the first day of August, at supper hour, the Burgundians took Messer Guglielmo d'Asciesi, the *conservadore* of the tyranny of the duke of Athens, and his eighteen-year-old son, Messer Gabriele. The latter had been recently knighted by the duke, but he was truly villainous in torturing citizens. They pushed him out of the gate of the palace into the arms of the enraged *popolo*, especially the friends and relatives of those his father had executed: Altoviti, Medici, Oricellai, Bettone Cini, and many others. To increase the father's pain, the son was shoved out in front, and they dismembered him and cut him into minute pieces. This done, they pushed out the *conservadore* and did the same. Some carried a piece on a lance or sword throughout the city. And there were those so cruel and bestial in their fury that they ate the raw flesh. Such was the end of the traitor and persecutor of the *popolo* of Florence. And note, he who is cruel dies cruelly, *dixit dominus*. This furious revenge having been accomplished, the anger of the *popolo* was much quietened, which was the salvation of Messer Cerrettieri. He should have been the third, and well he deserved it. . . . But let us return to our subject of the affairs of the duke. The Sunday following, the third day of August, the duke surrendered and gave the palace to the bishop, the fourteen, the Siense, and Count Simone.⁹

Villani's account provided the substance of all of those that followed, but the chronicle of Marchionne Stefani, written circa 1380, a generation after Villani's death, already shows striking changes in language and interpretation. Stefani shifted the moral and political responsibility for what happened from the duke to the Florentine *grandi*. Whereas Villani saw the duke as both needy and greedy, Stefani, taking more seriously Walter's lineage and titles, argued that such a man had no need to accept the relatively small rewards originally offered him as Florence's captain. Stefani assumed, therefore, that the duke was only waiting for the fulfillment of promises secretly made to him by the *grandi*. Villani had accepted the existence of social classes in Florence almost as a fact of nature. But in his view each of the classes, as represented in successive factionally dominated regimes, had failed to provide proper political leadership for Florence. Stefani, however, was particularly critical of the *grandi*, who possessed unique advantages and, therefore, unique power to do

⁹ Giovanni Villani, *Cronica*, ed. Dragomanni, 4 (Florence, 1845): 35: "Alla fine nulla concordia assentio il popolo, se non avessono dal duca il conservadore, e il figliuolo, e messer Cerrettieri Visdomini per farne giustizia. Il duca in nulla guisa l'assentiva, ma i Borgognoni ch'erano assediati in palagio s'allegarono insieme, e dissero al duca, che innanzi che volessono morire di fame e a tormento, darebbono preso lui al popolo, non che i detti tre, e ordinato l'aveano, e aveanne il podere di farlo, tanti ve n'erano, e si v'erano forti. Il duca veggendosi a tale partito acconsenti; e il venerdì, il primo di d'Agosto, in su l'ora della cena, i Borgognoni presono messer Guglielmo d'Asciesi, detto conservadore della tirannia del duca d'Atene, e un suo figliuolo detto messer Gabbriello d'età di diciotto anni, e di poco fatto cavaliere per lo duca, ma bene era reo e fellone a tormentare i cittadini, e pinsonlo fuori dell'antiporto del palagio in mano dell'arrabbiato popolo, e de' parenti e amici di cui il padre avea giustiziati, Altoviti, Medici, Oricellai, e quegli di Bettone Cini principali, e più altri, in presenza del padre per più suo dolore, il suo figliuolo pinto fuori innanzi il tagliarono e smembrarono a minuti pezzi; e ciò fatto pinsero fuori il conservadore e feciono il simigliante, e chi ne portava un pezzo in su la lancia e chi in su la spada per tutta la città; ed ebbonvi de' sì crudeli, e con furia sì bestiale e tanto animosa, che mangiarono delle loro carni crude. E cotale fu la fine del traditore e persecutore del popolo di Firenze. E nota, chi è crudele crudelmente more, dixit Dominus. E fatta la detta furiosa vendetta molto s'acquietò e contentò la rabbia del popolo; e fu però scampo di messer Cerrettieri, che dovea essere il terzo, e bene lo meritava. . . . Ma torniamo a nostra materia de' fatti del duca, che la domenica appresso, di 3 d'Agosto, il duca s'arrendè e diede il palagio al vescovo e a' quattordici, e a' Senesi e al conte Simone. . . ."

harm. The *grandi*, he wrote, were few in number and naturally respectful of their elders, which enabled the *grandi* to discuss, decide, and act. For the *populi* this was impossible, since they were too numerous to be brought together in any one place. Consequently, they were credulous and easily led by a tap on the shoulder or a suggestion whispered in the ear.¹⁰

In moving away from Villani's emphasis on the culpability of Walter, Stefani employed a morally charged language with a fury that far exceeded anything in Villani. The *grandi*, hoping to overthrow the Ordinances of Justice and to avoid payment of debts incurred in the recent bankruptcies, tried to aggrandize themselves over the little sheep and their shepherds. Like wolves they wanted to kill the sheep and discard the skin and eat the flesh and make dice of their bones. Without qualification Stefani condemned the nobles and great families as the enemies of humankind—*li nemici della umana spezie*.¹¹ Obviously, Stefani drew on the potent language of the Christian tradition, but does this mean that, like Villani, he perceived history and politics in essentially moral terms? Evidently not, for in the next phrase Stefani turned to an image of struggle drawn from nature, which recalls his earlier sociological analysis of number: it has always been so, and not only among the Florentines, since big fish have always eaten the smaller.¹² Clearly, the inevitability of conflict reduces the effect of the moral judgment, which is relegated to rhetoric. If the few always have advantages over the many, if the big fish always eat the little, in what sense can those who act as their number and nature dictate be condemned as "enemies of humankind"? For Villani, who believed in the literal conflict of God and the Devil in history, Satan alone was the enemy of mankind. But what was literal in Villani became metaphorical in Stefani, who rendered his partisan anger as biblical moralism because that was the most evocative language he knew.

This combination of sociological substance and moralistic rhetoric appears a number of times in Stefani's account. Thus, the failure of the duke's regime was attributed to his unwise decision to alienate the *grandi* and woo the populace. He forgot, wrote Stefani, that the people crucified Christ, crying "Let Him die, let Him die." The duke should have remembered that they would serve him no better than they had served Christ, who was a just "Signore." Meanwhile, the *grandi*, having been the chief proponents of the duke, thought that they could say "‘*noli me tangere*’ e ‘*nec tangere Christos meos*.’"

¹⁰ Stefani, *Cronaca fiorentina di Marchionne di Coppo Stefani*, ed., N. Rodolico, in *Rerum Italicarum Scriptores*, 30, pt. 1 (Città di Castello, 1903): 194: "Il popolazzo ed ancora i mezzani, che non vivono con niun ordine, e perocchè sono troppi a ragunarsi, o ad intendersi, s'imbeccano per gli orecchi, o per esser loro toccato la spalla, e col presente lusingamento senza nullo provvedimento di futuro da loro, o d'altronde non è chi loro lo mostri, s'accordano a chi loro parla, e credenti sono; li Grandi di senno, di gentilezza, d'ordine e le famiglie, che hanno sempre reverenza a uno il più savio del loro legnaggio, o a pochi, è poco accordare, e discutono le loro faccende, e veggonne il meglio; dico, de' loro appetiti non il meglio sempre, ma la volontà degli appetiti accordano piuttosto, o bene, o male, che pigliano, perchè hanno meno a consigliare ed a ragunare; ai popoli, come detto è, è impossibile."

¹¹ Stefani, *Cronaca*, 194: "Ma egli aspettava quello che li nimici della umana spezie, cioè li Grandi e le famiglie gli prometteano, chi per non pagare a cui avieno a dare, e chi con credendosi levare gli ordini de' Grandi d'addosso, e farsi maggiori sopra le pecorelle e i pastori delle pecorelle, e come lupi tonderle e vender la pelle, e poi mangiarsi la carne, e dell'ossa far daddi. . . ."

¹² Stefani, *Cronaca*, 194: ". . . come sempre fu, chè non è questo vizio de' Fiorentini solo, fu che sempre i pesci maggiori mangiano li minori."

But the *grandi* were wrong, Stefani concluded, since a *signore* or tyrant usually oppresses those who gave him power.¹³ Thus, for all its biblical reference, Stefani's analysis again concluded with a stark observation on the realities of political behavior.

The same combination of qualities recurred in Stefani's description of the murder of the duke's henchmen. His account of this key moment in the story differs little from Villani's in detail but lacks Villani's conviction of the fitness of the punishment. Stefani recorded the death of the eighteen-year-old youth, for example, without any judgment on his guilt or innocence. And, in describing the tearing, cutting, and biting of the bodies of the victims, he added that, according to what one reads, souls in hell do not fare worse.¹⁴ Stefani thus amplified his description of the torments of the body by reference to the agony of the soul, not because one is the fixed consequence of the other, but because the reference added drama to the scene. Clearly, he saw politics as a crude struggle and employed traditional moral imagery to express, not to justify, its harshness.

A century after Villani, Leonardo Bruni, the foremost Florentine civic humanist, composed his *History of the Florentine People*, which he presented to the republic in 1439. Though he did not in any way diminish the importance of the episode of the duke's tyranny and expulsion, he completely transformed the earlier account. Casting aside Villani's providentialism, Bruni focused on the political and constitutional core of the crisis. In so doing, he invested the action and his own narrative with a new rationality and clarity. Rather than ascribe the duke's ambition to depravity, for example, Bruni explained that, since the duke was a Frenchman, he was used to a servile people and regarded popular rule as ridiculous. The historian focused on the motives and actions of the duke and treated every move as a choice between clear alternatives or a well-considered step toward political goals. As a result, attention is directed away from the actuality of events, away from events as facts, and toward an examination of motives.

Bruni paid a price for this rational approach to narrative. What this price was and why he was willing and even anxious to pay it emerges clearly from his description of the murder of the duke's henchmen—the moment that Villani had made the most vivid of the story. This scene, described with such sensuous immediacy by Villani, almost disappears in Bruni's account, while around it grows a constitutional concern unstressed in the chronicle:

But these things did not calm the city. Especially those whose kin had been killed

¹³ Stefani, *Cronaca*, 199: "E non avea a memoria che crocifissero Cristo, gridando: 'Muoia, muoia.' Ben dovea egli avere a memoria che non farebbono meglio a lui che a Cristo, che fu giusto Signore." He continued with reference to the Grandi: "... e pareva loro poter dire 'Noli me tangere,' e 'nec tangere Cristos meos' erano ancora ingannati, imperocchè a rado si vide mai signore, o tiranno, che chi gli dà la signoria, egli non lo iscemi quanto può, perocchè dice: 'Come me la diede, me la può torre.' " For good measure, Stefani concluded the thought saying that God "per miracolo" made him bear the penalty for suppressing liberty.

¹⁴ Stefani, *Cronaca*, 209: "... e fu gittato fuori della porta il figliuolo del conservadore, il quale avea 18 anni ed appresso lo conservadore. Il popolo bestialmente straziando, e tagliando questi, chi con un pezzo, e chi con un altro n'andava via, e chi ne mangiava, e chi ne mordea, che, secondochè si legge, in inferno non si fa peggio di un'anima. Ed assai vituperevole cosa era a vedere.

burned with desire for revenge, nor did they judge it possible to expiate the murder of their own without the tyrant's blood. In order to impose some order on the multitude, which have taken up arms without any public resolution or legitimate leader, a council of the people, called by the leading citizens, met at Santa Reparata. By their vote fourteen men were elected with authority to reform the republic and give order to the city. One more was added: Bishop Angelo Acciaiuoli, a man of great wisdom and authority who was virtually the leader in the recovery of liberty.

Meanwhile, the siege and battle did not cease by day or by night. With the tyrant was a strong force of nearly three hundred men and the palace was very well provisioned. But these measures seemed only to delay so great a threat, not to give hope of safety. Therefore, the besieged first urged talks with the citizens, then implored their protection, finally begging and entreating. To placate the anger against the tyrant a few were offered up in sacrifice. Revenge was demanded against the duke's ministers who lately revelled in punishing citizens. They were ejected through the gate onto the swords of the eager populace. There, torn limb from limb, they received the fitting reward of their cruelty.

The anger of the citizens thus quietened, the bishop and the Fourteen began to negotiate.¹⁵

How ruthlessly the humanist suppressed the violence of his source. For Bruni the victims remained nameless and met their fate without rhetoric or pathos. He left unrecorded the struggle between the duke and his soldiers or the hunting down of the duke's ministers. Here the fury of a people literally savoring their revenge was reduced to swift justice. The humanist historian, seeking to revive the decorum of history, turned away from such a repugnant scene. Equally, he was especially anxious to protect the dignity of the Florentine republic. The state rather than the community was his chief concern, and the spectacle of the community in arms without a legitimate leader or public resolution, a sight that had stirred Villani's pride, only worried Bruni. Thus, he raised the bishop, as the one legitimate leader, to an unprecedented prominence and emphasized the re-establishment of the constitutional order.¹⁶

Let us now return to Machiavelli's history composed circa 1524:

The citizens met in Santa Reparata to draw up a constitution. They elected fourteen citizens, half nobles and half *popolani*, who would have absolute authority with the

¹⁵ Leonardo Bruni, *Historiarum florentini populi*, ed. Santini, in *Rerum Italicarum Scriptores*, 19 (1836): 163: "Sed ob eas res nihilo magis pacata civitas est. Urebat enim mentes ulciscendi libido, eorum praesertim quorum ile propinquos agnatosque necarat, nec sine sanguine tyranni expiari posse caedem suorum arbitrabantur. Ut tamen multitudo, quae, sine ullo publico decreto, sine ullo duce legitimo in armis erat, modum aliquem formaeque susciperet, principibus vocantibus, concio populi convenit ad Reparatae aedem. Ibi per suffragia quatuordecim viri cum imperio delecti sunt ad rempublicam conformandam ordinandamque civitatem. Additus est iis Angelus Acciaiuoli praesul, vir summo consilio summaque auctoritate, qui fere princeps fuerat libertatis recuperandae. Obsidio inter haec et oppugnatio neque die neque noctu intermittebatur. Sed erat cum tyranno manus valida militum ferme trecentorum; locus vero egregie munitus. Verum haec talia crant, ut procrastinationem tantum periculi, non autem spem ullam salutis viderentur afferre. Quare, modo colloquia civium poscere, modo fidem implorare, supplicare denique et obtestari obsessi pergebant. Quin etiam, quo piaculo aliquo delineretur ira tyranni, satellites quondam in civium suppliciis debacchatos, quos ad vindictam deposci sentiebat, per vim porta detrusos gladiis ardentis populi obecerunt: qui, illico membratim discerpti, fructum crudelitatis suae dignissimum reportarunt. Ex hoc jam restincta parumper civium ira, praesul et quatuordecim viri colloquia magis iniverunt."

¹⁶ Bruni's magnification of the role and dignity of Bishop Acciaiuoli is striking. Villani and Stefani had depicted him as weak and vacillating and, for much of the story, a willing supporter of the tyrant. For Bruni, however, Acciaiuoli's high rank and office provided a much needed source of legitimacy that could give order to these unruly events.

bishop to reform the constitution of Florence. They tried to negotiate a settlement between the people and the duke. But the people refused to hear any talk of an agreement before Guglielmo d'Ascesi had been handed over to them with his son and Cerrettieri Bisdomini. The duke did not want to agree, but he let himself be persuaded by the threats of those who were shut up with him. Indignation seems greater and injuries appear more serious when liberty is being won back than when it is being defended. Guglielmo and his son were let loose in the midst of thousands of their enemies; the boy was not eighteen; yet his age, figure, and innocence could not save him from the fury of the mob. Those who could not strike them while they were alive struck once they were dead; and, as if it were not enough to hack them with steel, they tore at them with their hands and teeth. After they had heard their shrieks, seen their wounds, touched their torn flesh, in order to let all their senses enjoy their revenge, they still wanted taste to savor it, so that, after all the external organs, the internal ones should be satisfied too.¹⁷

This is a skillful combination of elements from both Bruni and Villani. Machiavelli accepted the constitutional framework Bruni had created, but within it he restored the scene of violence that Bruni expunged. Yet, for all the sensuality of his description, he established a sense of order absent from Villani's vivid narration. Machiavelli's observation that passions run higher in the recovery of liberty than in its defense forecasts and explains the violence to come, while its abstractness and irony distance us from the scene. Far from expurgating the violence that follows, Machiavelli seems to have relished it; but at the same time he gave it a kind of logic by constraining it within an orderly sequence of the senses—hearing, seeing, touching, tasting—as though the fury of the people were being voided by a kind of controlled release. Similarly, brutality is turned to pathos by Machiavelli's transformation of the slaughtered boy into an innocent and beautiful youth.

Machiavelli's "reawakening of the directly sensible," to revert to Auerbach's phrase, was more than rhetorical. It expressed a renewed feeling for the particularity of Florentine history.¹⁸ Villani's intense localism had been

¹⁷ Machiavelli, *Istorie*, 113–14: "I cittadini, per dare forma allo stato, in Santa Reparata si ridussono; e creorono quattordici cittadini, per metà grandi e popolani, i quali con il vescovo avessero qualunque autorità di potere lo stato di Firenze riformare. . . . Costoro intra il popolo e il duca alcuna convenzione praticorono; ma il popolo recusò ogni ragionamento d'accordo, se prima non gli era nella sua potestà dato messer Guglielmo d'Ascesi, e il figliuolo insieme con messer Cerrettieri Bisdomini consegnato. Non voleva il duca acconsentirlo; pure, minacciato dalle genti che erano rinchiusse con lui, si lasciò sforzare. Appariscono senza dubbio gli sdegni maggiori, e sono le ferite più gravi, quando si recupera una libertà, che quando si difende. Furono messer Guglielmo e il figliuolo posti intra le migliaia de' nimici loro; e il figliuolo non aveva ancora diciotto anni: nondimeno la età, la forma, la innocenza sua non lo poté dalla furia della moltitudine salvare; e quelli che non poterono ferirgli vivi gli ferirono morti; né saziati di straziarli col ferro, con le mani e con i denti gli laceravano. E perché tutti i sensi si sodisfacessero nella vendetta, avendo udito prima le loro querele, veduto le loro ferite, tocco le loro carni lacere, volevano ancora che il gusto le assaporasse, acciò che, come tutte le parti di fuori ne erano sazie, quelle di dentro ancora se ne saziassero." Translation in the text by Dunne, *History of Florence* (New York, 1960), 100.

¹⁸ In the epilogue to his valuable book on Bruni, Wilcox has written that a "close reading" of Machiavelli's "account of Walter of Brienne shows clearly that his basic interpretation is taken directly from Bruni's *Historiae*. The difference between Machiavelli and Bruni in this example is not analytical but rhetorical. Machiavelli successfully incorporates sensory elements and provides a more convincing picture of the event, while Bruni's account is wholly devoid of references to sensory objects. If this difference obtains throughout Machiavelli's history, then his true accomplishment takes on a somewhat different complexion, especially in view of Scala's failure to develop an effective appeal to the senses." Wilcox, *Humanist Historiography*, 203. The observation is correct, but Wilcox has failed to acknowledge the source of

enriched by a providential vision that gave moral significance to every event of Florentine history. Bruni, by contrast, sought to endow the history of Florence with greater dignity and moral value by constructing local events, as far as possible, according to a classical model. Machiavelli, however, joined the general to the particular by putting universal considerations to work to understand and dramatize the particulars of the Florentine situation. In the speech of the priors, which preceded these events, for example, the general problem of the suppression of liberty was given special application by the judgment that Florentines were even more liberty-loving than others.¹⁹ Thus, Machiavelli's interest in the concreteness of Villani's account expressed his most serious purposes. As he wrote in the "Proemio," the causes and consequences of faction are the most important political lessons for men who govern republics, and Florence, more than any other republic, was outstanding for its history of civil discord.

MACHIAVELLI WAS A MEDIATING FIGURE in Florentine historiography. A native Florentine and republican, he was also a secretary, commissioned historian, and man of letters. By contrast, Guicciardini was a patrician politician who, by interest and instinct, belonged to the traditional commercial aristocracy. More than Machiavelli, Guicciardini recalls the type of the merchant chroniclers; and the more we regard Florentine historiography from the perspective of Guicciardini, the more important the vernacular tradition becomes.

The greatest single historical work written by a Florentine was certainly Guicciardini's masterpiece, *The History of Italy*. Here for the first time the narrow horizon of Florentine affairs widened to encompass all of Italy. Unfortunately for our purposes, the extent to which this work departed from the traditional focus on Florence itself frustrates the kind of specific comparison with earlier works that is possible for Machiavelli's account. Nevertheless, a review of Guicciardini's career as a historian will demonstrate that at every stage Guicciardini's writing shows marked features that were alien to the classicizing practices of the fifteenth-century humanists.²⁰

Early works reveal writers' inherited ideas and practices. Significantly, Guicciardini's first works incorporated a range of interests and forms that had remained essentially unchanged since the days of trecento merchant chroniclers. Family memoirs, civic history, personal *ricordanze*, moral and political

this sensory side of Machiavelli. Machiavelli's feeling for the vivid and the immediate in Florentine history was far more than a rhetorical choice; it is a sign that he enjoyed a different relationship to the city's history than did Bruni. Another commentator has noted that Machiavelli concluded with a physical description of the villainous duke—without, however, acknowledging that the description is taken over entirely from Villani. Bondanella appears to suggest, therefore, that the description represents a free invention on Machiavelli's part: "Such symbolic physical description, hardly intended as an accurate account of the man's appearance, is the perfect ending for the sketch of the perfect villain"; *Machiavelli*, 104–05. The dangers of ignoring the vernacular background are evident.

¹⁹ Machiavelli, *Istorie*, 103.

²⁰ For what follows, but set out in greater detail, see my *Guicciardini*. Felix Gilbert's *Machiavelli and Guicciardini* and his essays in *History: Choice and Commitment* remain the most powerful interpretation of Guicciardini and his context, and I have been greatly influenced by these works.

maxims, and political dialogue—these were the writings of Guicciardini as a young man. All were composed in traditional genres, which he used without self-consciousness or apology, a sure sign that they did not strike him as unsuitable or outdated.²¹ This persistence of old habits of self-expression, especially in one so ambitious and gifted as Guicciardini, seems convincing evidence that vernacular traditions had survived the growing prestige of classicism in the fifteenth century.

Perhaps the point will be clearer if turned around: let us try, for example, to imagine Leonardo Bruni beginning his historiographical career not with the *Laudatio florentini urbis* but with the *Memorie di famiglia*. It is true, of course, that even in his early *History of Florence* (ca. 1508) Guicciardini was aware of humanist models, and in some passages—the well-known portrait of Lorenzo Magnifico, for example—he was able to make good use of them.²² But these passages are exceptional in a narrative that is looser and more colloquial than classical decorum allowed. *The History of Florence* remains the work of a citizen, not a man of letters. It is most revealing that Guicciardini composed his history, like his family memoir, from papers in his family's possession. Like all works in the vernacular tradition, it has the concreteness that is the mark of the author's intimacy with his own city.²³

In later works Guicciardini's acquaintance with classical models deepened, and in some ways his affinity with them grew. His diction was more elevated; orations, which had been absent from his first history, became a regular feature of his work; and, most important, his sense of narrative form progressed remarkably. But with this maturation, his self-consciousness as a student of politics and history also developed, and the differences that separated him from his humanist predecessors became conscious and critical. In the terse, knotted formulations of the *Ricordi*, he reached a new sense of the inherent limits of political and historical knowledge. Indeed, he emphasized these limitations to such a degree that understanding even a single event became problematic and generalization impossible.²⁴ All circumstances are unique and any variation, no matter how trivial, might be decisive. Analogies are dangerous and no prescription valid, except one that counsels caution. For Guicciardini the lessons of history gave way to the rules of prudence, and

²¹ Velluti's domestic chronicle, Dati's secret book and historical dialogue, Morelli's diary, Lapo da Castiglionchio's dialogue called the *Epistola*, and Capponi's *Ricordi* are examples of this tradition. On this literature, see Bec, *Les marchands écrivains*. Guicciardini's earliest works are the *Memorie di famiglia*, the *Storie fiorentine*, the early *ricordi*, the *Ricordanze*, and the *Discorso di Logrognio*. See Guicciardini, *Opere*, ed. Palmarocchi (Bari, 1932), and *Ricordi*, ed. Spongano (Florence, 1951).

²² One indication that Guicciardini saw himself in some kind of succession to the chancellor-historians is his statement early in the history that from the Peace of Lodi on he would begin to narrate more fully, since no one had written the history. Guicciardini, *Storie fiorentine* (Bari, 1960), 8.

²³ See N. Rubinstein, "The *Storie fiorentine* and the *Memorie di famiglia* by Francesco Guicciardini," *Rinascimento*, 4 (1953): 171–225. In connection with this family focus I should note Machiavelli's remark in the "Proemio" that, if Bruni had suppressed the history of civil disorders in order to spare the families, he was mistaken, since most people are anxious to have the deeds of their ancestors known. Guicciardini's reference to his ancestor Luigi Guicciardini, who played a most unheroic role in the Ciompi revolt, seems to bear Machiavelli out. Bruni, of course, kept to a bare minimum the number of individuals mentioned, whereas for Guicciardini it would have been inconceivable to leave them out of his history.

²⁴ See M. Fubini, "Le quattro redazioni dei Ricordi del Guicciardini," in *Studi sulla letteratura del Rinascimento* (Florence, 1948); and Phillips, *Guicciardini*, chaps. 4–6.

historical writing was freed from any obligation to provide moral or political instruction. Thus, the didacticism that was fundamental to classical historical thought—and to Machiavelli's as well—was swept away.

By approximately 1530 when he wrote the cluster of works that mark the middle of his career—the late redaction of the *Ricordi*, the *Considerations on the Discourses of Machiavelli*, and the *Cose fiorentine*—Guicciardini had set a new course for himself and for Florentine historiography. His repeated criticisms of Machiavelli's historical simplifications, his rejection of any analogy between Florence and Rome, and his insistence on particularity in history amount to a denial of the very things that historians in the classical tradition had always cited as giving value to history. And the consequences of this position for historical writing began to appear in the unfinished *Cose fiorentine*, Guicciardini's second attempt at a history of Florence.

The most remarkable aspect of this fragmentary work is the author's evident struggle to examine—and examine critically—all of the available sources for Florentine history, vernacular and humanist.²⁵ References to chronicles abound, as do admonitions to himself to investigate aspects of the story for which his evidence was still incomplete. As with Machiavelli, Guicciardini's increased awareness of the historiographical tradition in Florence had led him back to the chronicles and diaries that were the first sources of that tradition. But Guicciardini had not lost sight of his classical inheritance either. Carefully composed orations manifest his intention of adding to his painstaking narrative another sort of authority. Together the critical notes on sources and the orations can stand for the impulses at work in the *Cose fiorentine*. What the final result might have been we cannot know, but as an unfinished structure this work reveals even more clearly the opposed elements its author was struggling to reconcile.

That reconciliation came fifteen years later in Guicciardini's *History of Italy* (1536–40). His mature style avoided colloquialism and sensuous detail; but, on another level, the loss of vividness is more than compensated by an overwhelming richness of incident. In a narrative of great complexity, Guicciardini produced a concrete investigation of an entire generation of Italian political life. This vast yet detailed record of the tragedy of Italy is, in a sense, both chronicle and history, but by now Guicciardini's historical art had far outstripped his models.

AS STATED AT THE OUTSET, Florentine historiography has generally been seen as evolving in three progressive stages: preclassical, classical, postclassical. But this view is not easy to reconcile with the persistence in Machiavelli and Guicciardini of strong vernacular impulses. Where, after all, do these impulses come from if, as is often thought, vernacular historiography faded away in the quattrocento or became merely trivial? We must suppose, then, that the writing of vernacular chronicles did not die out but remained a living tradition

²⁵ See G. Ridolfi, ed., Guicciardini's *Cose fiorentine* (Florence, 1945), Introduction.

vital enough to become a major source of strength in the work of Machiavelli and Guicciardini.

A close reading of the two great sixteenth-century Florentine historians suggests that substantial continuity of vernacular historiography in the fifteenth century is plausible. But to press the point further, it would be useful to have more positive evidence, which requires a survey of the relevant vernacular accounts. A full survey would be impossible here, but even a short one reveals that a considerable number of texts can be cited for the first half of the century and for its last decade—that is, for the early period of Medici domination and for the years after their expulsion. The weakest link, and therefore the period most needing investigation, lies in the decades after the middle of the century, when the Medici hegemony was fully established.

In the first half of the quattrocento vernacular historiography continued to flourish, as the works of Dati, Capponi, Buoninsegni, and Cavalcanti make plain.²⁶ This is no surprise, certainly, for one would not expect the new classical style to triumph abruptly and completely. After mid-century, however, there appears to have been little chronicle-writing. A renewed outpouring of vernacular narratives followed the expulsion of the Medici in 1494, leaving a vivid picture of the troubled years of Savonarola and the republic. Since the latter period has been extensively studied by Felix Gilbert, I will do no more than relate some of his conclusions to our present concerns. Examining the histories of Cerretani, Piero Parenti, and the young Guicciardini against the background of the political and military crisis that Florence faced in these years, Gilbert has seen these patricians as searching for a more pragmatic historiography. They subordinated history to politics and in various degrees modified or abandoned the humanist pattern of history in order to incorporate in their narratives the political concreteness that pragmatic history required. In the end this search led to the new political and historical perspectives of Machiavelli and Guicciardini.²⁷

Gilbert's work leaves no doubt that the French invasion and the expulsion of the Medici had profound consequences for the development of Florentine thought. At the same time, as with any crisis, much of what occurred can also be seen as building on and giving renewed emphasis to existing traditions. Thus, from the perspective of this study it seems reasonable to view the pragmatic historiography that developed after 1494 as an extension of the concreteness and particularity that always characterized vernacular writing.²⁸ A certain directness of approach is, after all, the mark of the citizen dealing with the affairs of his own community. It is no accident, then, that the men

²⁶ On this literature, see Varese, *Storia e politica*; and Bec, *Les marchands écrivains*.

²⁷ Gilbert, *Machiavelli and Guicciardini*, esp. 226–35.

²⁸ Gilbert was certainly aware of the vernacular antecedents; he has remarked that Parenti's work came out of the diary tradition while Guicciardini's had roots in the family memoir. Although he stated that neither classical nor vernacular forms could meet the new needs of these writers, Gilbert has stressed the importance of the humanist background: "It is certainly true that the emergence of a new view of the purpose of history was due to the influence of the humanists. They had infused into the concern with the past a new significance by recognizing history as an important literary genre which could give man guidance in his actions"; *Machiavelli and Guicciardini*, 228.

Gilbert has singled out were all natives of the city. Unlike Bruni or Poggio, nonnatives who made careers in the service of the state, these patricians were tied to the Florentine community by birth, marriage, political ambition, family status, and commercial enterprise. Finally, it is consistent with all of this that most of the histories written by these patricians were decidedly unclassical in form. Though there is ample evidence of the influence of the classical vocabulary in each, neither Piero Parenti's historical diary nor Guicciardini's Florentine history is classical in inspiration. And to these we can add the chronicle of Cambi, another major source for this period. Only Cerretani attempted, however unsuccessfully, to write "true history."

There remains a critical difficulty: the apparent interruption in the line of continuity after the middle of the fifteenth century. In this period of Medici consolidation, it might be supposed that the prestige of humanism, which had become the unofficial court culture of the Medici, smothered humbler *volgare* genres, whose roots lay in the communal past. Admittedly, classicizing historiography did not flourish either, but the vernacular was just as thin.²⁹ *Ricordanze*, diaries, chronicles, and *prioristi*, do exist, of course, for this period, and historians of the Medici hegemony have found them useful sources.³⁰ Few, though, would regard such records, if taken by themselves, as the proper ancestors of Guicciardini's *History of Italy*.

Two works, however, seem significant exceptions to this rule, one long known but often disregarded, the other only recently identified. They are the chronicles of Alamanno Rinuccini and Marco Parenti, two Florentines who found in vernacular historical writing a way of expressing hostility to the Medici regime. Alamanno Rinuccini, to begin with the better known of the two, was a descendant of an ancient family and a member of Florence's political and intellectual elite. Best known for his Latin dialogue on liberty, Rinuccini has been described by Hans Baron as the direct heir of civic humanism.³¹ But Rinuccini was also the author of a *priorista*, a highly traditional chronicle form in which notices of events are appended to chronological lists of the priors, a civic variant on medieval chronicles organized by papal or imperial reigns. His father had begun the work, making of it only a rather trivial civic diary largely concerned with the ceremonies of communal life in the first half of the fifteenth century. After his father's death in 1461, Alamanno continued the *priorista*; and, though he did not alter its traditional form

²⁹ After Bruni and Poggio humanist historiography certainly suffered a decline, both in quantity and quality. See N. Rubinstein, "Bartolomeo Scala's *Historia florentinorum*," in *Studi di bibliografia e di storia in onore di Tammaro de Marinis*, 4 (Verona, 1964): 49–59; and D. Wilcox, "Matteo Palmieri and the *De captivitate Pisarum Liber*," in Molho and Tedeschi, *Renaissance Studies in Honor of Hans Baron*, 265–82. Baron, too, has commented that "for Florentine historiography and political philosophy the era of Lorenzo de' Medici, with its restriction of civic liberty and vitality, was an interval of decay. . ."; *Crisis of the Early Italian Renaissance*, 342.

³⁰ For reference to these, see N. Rubinstein, *The Government of Florence under the Medici* (Oxford, 1966), esp. xii n.

³¹ Baron, *Crisis of the Early Italian Renaissance*, 437. Baron has designated Rinuccini and Donato Acciaiuoli, the translator of Bruni's history, as "the two principal heirs of civic Humanism." For Rinuccini the humanist, also see Rinuccini, *Lettere ed Orazioni*, ed. V. Giustiniani (Florence, 1953); E. Garin, *Italian Humanism*, trans. Munz (Rome, 1960), 73–81; and F. Adorno, "La crisi dell'umanesimo civile fiorentino da Alamanno Rinuccini al Machiavelli," *Rivista critica di storia della filosofia*, 7 (1952): 19–40.

or language, he transformed it with a passion for civic freedom and hatred of the Medici “tyranny.”

Rinuccini held a succession of high posts and was a well-informed observer. Above all he constituted himself a witness, both passionate and sophisticated, to the consolidation of the Medici regime under Piero and Lorenzo. In places his chronicle is a single-minded record of the electoral manipulations that protected the regime. One typical case may be cited. In 1480 the priors, “without ringing the bells or other sign,” called an assembly (*parlamento*). By means of complicated electoral procedures, which he described in full, thirty men were given special powers as a *balia*. Rinuccini’s condemnation was unequivocal: like the Athenians, the Florentines had lost all liberty and were reduced to subservience to the tyranny of the thirty.³² Rinuccini’s protest at the absence of the legitimating symbols of the commune might have been voiced by any trecento chronicler. His comparison of current oligarchs to the Thirty Tyrants of Athens, on the other hand, belonged most clearly to a new age—a sign of classical influence penetrating even a genre as unclassical as the *priorista*. In the last analysis, though, Rinuccini cited his own presence, not classical precedent, as the guarantee of the authenticity of his message. “And this I say truly,” he wrote, “although I, Alamanno Rinuccini, was appointed one of the number of this *balia*.” That unusual and significant “although,” which in a less complex chronicler would have simply been “because,” signals the irony and discomfort of this witness to Florence’s loss of freedom—and his own.

For all its interest, however, Rinuccini’s chronicle has not been taken seriously as a historical work. Its old-fashioned character and episodic structure have attracted much less attention than the literary Latin of the dialogue on liberty. It is, moreover, a brief work by comparison with the major chronicles of the trecento, although it records a full generation of Florentine politics. From several points of view, therefore, Rinuccini’s *priorista* may seem an isolated and anachronistic document, best explained as a minor effort motivated by filial piety. But this would be, I think, a serious distortion. There is no trace here of the private diary or *ricordanza*. The events Rinuccini records are fully public, as are his political passions. Nor was Rinuccini’s chronicle as exceptional as it has seemed until now; a second and essentially similar work of vernacular historiography, the chronicle of Marco Parenti, has recently been identified. This work, which has survived only in a later anonymous copy, must now be considered the major contemporary account of Florence in the 1460s.

Parenti belonged by right of wealth, marriage, and office to Florence’s

³² G. Aiazzi, ed., *Ricordi storici di Filippo di Cino Rinuccini dal 1282 al 1460 colla continuazione di Alamanno e Neri suoi figli fino al 1506* (Florence, 1840), 131–32: “Questi signori, a dì 8 di Aprile, senza suono di campana o altra dimostrazione feciono, si può dire, uno parlamento.” He continued below: “. . . donde s’intese o si potè intendere esser levata ogni libertà al popolo, e in tutto esser ridotto in servitute de’ sopradetti 30, come si legge esser già avvenuto ad Atene, di che seguí la rovina di quella citade e la perdita della libertate; e questo parlo per il vero, benchè io Alamanno Rinuccini fossi creato uno del numero di quelli della balia.”

governing class; and he seems to have been welcome in humanist circles.³³ Here is a second case, then, of a man of some consequence and education composing a decidedly unclassical history. His narrative is fuller and more continuous than Rinuccini's, but it remains an eyewitness chronicle. Despite ample signs that he had absorbed parts of the humanist vocabulary, Parenti showed no ambition to write "true history." On the contrary, the basic framework of his *ricordi*, as he called it at one point, is clearly revealed by a certain patient fondness for keeping lists.

This sub-literary preoccupation with preserving the record—be it personal, familial, commercial, or political, or a combination of them all—lies at the root of vernacular history. Villani's chapters listing the income and expenses of the commune, right down to the cost of wax for candles and "pasta" for the lions, are an early example. Closer to Parenti's own time, Benedetto Dei provided several striking examples of this love of lists. He decorated the covers of his manuscript with long columns of dialect words and, inside, he preserved the detailed measurements of the newly completed cathedral. More personally, he also included two lengthy lists of names, one headed "These are the friends of Benedetto Dei," the other "These are the proven enemies of Benedetto Dei."³⁴

Parenti's origins in this tradition are obvious. He, too, recorded the dimensions of the cathedral, and perhaps he approached the crankiness of Dei when he undertook to list all the seigneurs of Italy, starting at the top. But even so patient a chronicler balked at the innumerable nobility of Naples; he promised instead to write the names down in a small notebook to be inserted in the pages of the larger one. Lists of the noble houses of Venice and of the members of the College of Cardinals were also included.³⁵ And all of this was part of a loose and digressive narrative that clearly belonged to the vernacular type—though Parenti's apology for this lack of form indicates his awareness of the more literary tradition.

Parenti's enthusiasm for lists of foreign dignitaries could easily be dismissed if it were not joined to a very clear sense of the interconnectedness of Italian politics. In a lengthy retrospect on the rise of Francesco Sforza, for instance, he showed how closely Medici fortunes were tied to the Sforza. Many opposed Cosimo's aid to the Sforza, he wrote, wishing instead to support a republic in

³³ For what follows, see my "A Newly Discovered Chronicle by Marco Parenti," *Renaissance Quarterly*, 31 (1978): 153–60. Parenti was the son-in-law of Alessandra Strozzi and his connections with that family can be studied in the well-known collection of her letters: C. Guasti, ed., *Lettere di una gentildonna fiorentina del secolo XV ai figliuoli esuli* (Florence, 1877). For a brief biography of Parenti and his connections with humanism, see Lauro Martines, *The Social World of the Florentine Humanists* (Princeton, 1963), 346. Parenti was included in Martines's prosopographical survey of humanists, but the chronicle is in fact the only work of Parenti's to survive. The inclusion of men like Parenti suggests that Martines has provided us with a study of the influence of humanism among the Florentine upper class and not, as he has claimed, with an analysis of the social status of the humanists themselves. Martines's brief outline of Parenti's career needs to be corrected on one point that is essential to the identification of the anonymous chronicle: in addition to other communal offices that he held, Parenti was a prior in 1454.

³⁴ Benedetto Dei, "Cronica," *Archivio di Stato di Firenze, Manoscritti*, 119, C.44, f. 76r.

³⁵ BNF, Magliabecchi xxv, 272, pp. 4–14. [Hereafter Parenti's chronicle, which I am currently editing, will be cited as the *Ricordi politici*.]

Milan. In this way they hoped to restrain the power of the Medici and to create a strong equilateral triangle of free republics—Florence, Venice, and Milan—in which peace could be kept because any two could always counterbalance the aggressions of a third.³⁶ Thus, one generation before Bernardo Rucellai and two before Guicciardini, Parenti articulated the idea that Italy was a single, unified space in which peace could be maintained through a balance of power.

Parenti's interest in the interconnections of Italian politics grew out of this concern for the internal politics of Florence. His extensive account of the factional crisis that very nearly overthrew Piero de' Medici in 1465–66 is the most complete we have. Here, indeed, Machiavelli could have found a preoccupation with civil discord to match his own. Parenti's sympathies were with the anti-Medici faction led by Luca Pitti, which he styled "the party of liberty against tyranny." But irresolution and avarice paralyzed their cause, and many who might have followed them held back out of fear or self-interest. Only one man seemed bold enough to press the challenge:

Niccolo Soderini, a bold man as we have said, arrived at the house of Messer Luca [Pitti] on horseback, armed, and with many companions. There he found Messer Dietisalvi and Messer Agnolo and other honorable citizens. They were engaged in many discussions but had made few provisions suited to their needs. He told them that actions, not words, were required, and quickly too, before Piero strengthened his position. He asked that all those present who were armed, along with his own men, follow him. They would go to the houses of their friends who out of doubt and fear had remained hidden and would flush them out to join him and the others. Whoever refused would be treated as an enemy. And, having gathered together many men in this way, as seemed likely, he would ride through the city crying "Liberty" and call it to arms. With this following he wanted to rush to assault the house of Piero and by every possible means overcome him, put him to flight, and rout him entirely.

To everyone this plan seemed likely to succeed; but one doubt held them back. This was the fear of the common people stirred to arms. . . .³⁷

This consideration being most prudent, wrote Parenti, Soderini's plan was

³⁶ Parenti, *Ricordi politici*, p. 57: "... che per migliore modo di vivere, et più pace d'Italia e' fussi bene di spegnere la tirannia di Milano et fare surgere una terza libertà potente già cominciata, et fussi terza tra noi et Vinitiani, et come lo spatio tra Firenze et Vinegia, et da Vinegia a Milano, et da Milano a Firenze è quasi equedistante in forma di triangolo, così in ogni canto come un capo fussi una città potente a tenere ritta in pie la pace di Italia, contrapesando sempre la terza alle due che si volessino opporre."

³⁷ Parenti, *Ricordi politici*, pp. 80–81: "Stando le cose in questi termini, Niccolò Solderini, huomo animoso come innanzi dicemo di lui, giunse a cavallo, armato, con molti compagni a casa M. Luca, dove era M. Dietisalvi et M. Agnolo, et altri honorevoli cittadini; et trovandogli in molti ragionamenti et pochi provvedimenti atti al bisogno, disse loro che quivi bisognava fatti et non parole, et prestì, innanzi che Piero si fussi corroborato a suo modo. Però chiedeva che tutti quelli armati che erano quivi insieme con suoi lo seguitassino allhora, et colloro voleva andare a casa gli amici loro che stavano occulti per dubbio et per timore, et fargli isbucare a seguirarlo come gli altri, et chi non lo volessi fare, trattarlo come inimico. Et ragunato molta gente a questo modo, che così pareva verisimile, correre la terra gridando libertà, et farla levare tutta in arme; et con questo favore correre a casa Piero, et quivi assaltarlo, et con ogni modo opportuno vincerlo, pigliarlo, fugarlo, et in tutto isbaragliarlo. Parve a ognuno modo verisimile a riuscire, ma un dubbio gli ritenne. Questo fu il timore del popolo minuto commosso in arme. . . . Questa considerazione fu prudentissima però fu dinegato tal partito. Ma al bisogno loro fu vilissima, però che le cose erano venute a tale conditione che niuno altro rimedio havevano alla salute loro."

vetoed. But, he concluded, in their situation this was cowardly, since things had come to a point where no other remedy existed.

Parenti was capable of seeing events in perspective. He knew that the danger of a Ciompi-style revolt was real, but he thought it a risk that had to be run.³⁸ Despite his sophistication, however, he remained content with the role of the witness, recording only what he had seen or heard without exploring the psychological dimension that Bruni had developed. The defection of Luca Pitti, for example, was a crucial moment in this story, and Parenti was able to give the most detailed account of the secret negotiations that led to it. By his own admission, however, he could not go beyond the facts as they appeared to an outsider. "If there were disputes or disagreements, it is not known to me," he wrote, "but I do know the conclusions to which they came."³⁹

To write history in this way, one had to be close to it. Unfortunately, recording events in a society steadily becoming more exclusive could be difficult, as Parenti himself acknowledged. The death of Cosimo and the ex-

³⁸ It is worthwhile to compare Machiavelli's version of this same incident. He, too, saw Soderini as the most energetic supporter of liberty, but he believed that Soderini's failure to get others to act was a result of Luca Pitti's betrayal of their common cause: "Ma di tutti si mostrò più vivo che alcuno Niccolò Soderini, il quale prese l'arme e fu seguitato quasi che da tutta la plebe del suo quartiere, e ne andò alle case di messer Luca e lo pregò montasse a cavallo e venisse in piazza a' favori della Signoria che era per loro: dove senza dubbio si avrebbe la vittoria certa; e non volesse standosi in casa essere o dagli armati nimici vilmente oppresso o dai disarmati vituperosamente ingannato: e che a ora si pentirebbe non avere fatto che non sarebbe a tempo a fare; e che se e' voleva con la guerra la rovina di Piero egli poteva facilmente averla, se voleva la pace, era molto meglio essere in termine da dare, non ricevere, la condizioni di quella. Non mossonno queste parole messer Luca, come quello che aveva già posato l'animo ed era stato da Piero con promesse di nuovi parentadi e nuovi condizioni svolto. . . . Non potendo adunque Niccolò altrimenti dirlo se ne tornò a casa ma prima gli disse: 'Io non posso solo fare bene alla mia città, ma io posso bene pronosticarle il male: questo partito che voi pigliate farà alla patria nostra perdere la sua libertà, a voi lo stato e le sostanze, a me e agli altri la patria.' " Machiavelli, *Istorie fiorentine*, 350–51. The two accounts have in common an interest in the drama of this crucial moment when the anti-Medici faction, as one can see in retrospect, had to act decisively or lose; and the tempestuous Soderini appealed to both as the lost hero of the moment. Parenti portrayed Soderini's pleas with greater heat and directness. Machiavelli's version is less colloquial and the arguments are more rational. The prophecy of defeat and exile, which Machiavelli put into Soderini's mouth at the end of the scene, is, however, a classical touch that has no immediate equivalent in Parenti—though his later portrait of Luca Pitti as the betrayer betrayed is similar in the way it isolates a single pose or gesture for exemplary purposes. But the message that the decisive moment in the struggle had been allowed to slip away, which Machiavelli underlined with the prophecy, Parenti expressed more concretely by narrating an incident to prove how unprepared Piero de' Medici was at this point. In one important detail Machiavelli showed himself either less well informed than Parenti or willing to sacrifice detail to a brisk narrative. For Machiavelli, Pitti failed to respond to Soderini because he had already changed sides; Parenti, however, gave no indication that it was Pitti in particular who balked at violence and feared a Ciompi uprising. Indeed, he himself found fear on this score to be prudent, though too timid under the circumstances. Parenti, who apparently was well informed on the matter, placed at a later date the decisive interview in which Pitti was brought off. Thus, Machiavelli telescoped the events in order to give a more personal motive for the failure to act where Parenti found a broader (and more interesting) one—fear of the lower classes. The irony of Pitti's situation has also been reduced in Machiavelli's account, because Machiavelli showed Pitti as being offered the bribe of a marriage with the Tornabuoni from the start; Parenti claimed, however, that Pitti was deliberately misled with hints of a still higher connection, the marriage of his daughter to Lorenzo de' Medici himself. In other respects Machiavelli accepted a Medici view of events in contrast to Parenti. Machiavelli included the story of the attempted ambush of Piero, which Parenti dismissed as Medici propaganda. And Machiavelli attributed the round of bank failures to Dietisalvi Neroni's manipulation of Piero. See R. Hatfield, "A Source for Machiavelli's Account of the Regime of Piero de' Medici," in Myron P. Gilmore, ed., *Studies on Machiavelli* (Florence, 1972).

³⁹ Parenti, *Ricordi politici*, p. 86: "Se dispute o controversie s'hebbono nel parlare insieme non mi è noto, ma è mi noto l'effetto a che e' vennonno."

pectation of seeing a free city had led him to begin his chronicle, he wrote; but the disappointment of these hopes and the secrecies practiced by an unfree regime have made his *ricordi* discontinuous and clumsy.⁴⁰ Here, surely, is a major explanation for the slackening stream of historical writing in this period—and for its resurgence after 1494. With access to politics barred to all but a few, most eyewitnesses could not rise above the trivial, the ceremonial, or the merely personal.

Nevertheless, Parenti and Rinuccini did create substantial records of their times. Close enough to the political center to observe how the regime worked, they were also, in private at least, opponents of the Medici, who were moved to denounce what they saw. And it is hard not to assume some connection between the form of their protest, the vernacular chronicle, and the “good old cause” they supported. In the unliterary vernacular, the traditional language of Florentine citizen-politicians, they could directly express their immediate concerns. As so often before and since, history, it seems, was the last refuge of patriots.

How much can we draw from these two chroniclers? Some may doubt that two examples, however interesting, are sufficient to bridge the gap between early and late fifteenth-century vernacular historians. Room for questions certainly remains; equally, the picture may yet be clarified by the recovery of other narratives. But vernacular historians did not constitute a literary group, and no amount of research will ever uncover the sort of interlocking chain of individuals and influences that marks a school or movement. It would be senseless, for example, to try to establish a specific model or influence that prompted Guicciardini to compose his family memoir. The vernacular tradition was less a program than a habit of mind, and its vitality is shown in the persistence of characteristic values and attitudes. This is the importance of Parenti and Rinuccini, for they indicate the vigor and centrality of the vernacular in the second half of the fifteenth century. Reading them, we rediscover the force of vernacular historical writing on its way toward the triumphs of Guicciardini and Machiavelli.

There remains, finally, a line of continuity of the most tangible sort. In addition to his other contributions to Florentine historical writing, Marco Parenti also fathered Piero Parenti, already mentioned as the most extensive chronicler of the Savonarolan period. It is now evident that Piero Parenti followed in a family tradition, and from father to son there was substantial continuity of style and viewpoint.⁴¹ This should come as no surprise, for Rinuccini had done the same. Indeed, from Velluti's domestic chronicle to Guicciardini's family memoir, family traditions and family concerns were

⁴⁰ Parenti, *Ricordi politici*, pp. 69–70: “Seguinne quel che nel principio di nostri ricordi habbiamo narrato in questo libro che da la morte di Cosimo incominciammo, stimando per l'avenire havere a scrivere fatti di città libera et di cittadini da diventare buoni, stracchi dall servitù de' tempi passati; et non mi riuscendo, aspettavo pure tempo che venissi questa libertà, che appressandosi, et non ne agiugnendovi in tutto, rovinò; mi invili l'animo, et la difficoltà del sapere il vero per i segreti che riteneva a sé chi governava, mi ritrasse dalla diligenza et pensiero havevo fatto. Et però tali ricordi sono misti di molta negligenza et non continovati in tutte le cose occorse, benché dove più et dove meno secondo e' tempi.”

⁴¹ This will be the subject of a separate essay.

always at the center of the vernacular tradition. Only the city itself loomed larger in the narratives of its native historians.

I HAVE EXAMINED THE HISTORIOGRAPHICAL INHERITANCE of Machiavelli and Guicciardini not to narrow but to broaden it. In their works Florentine historiography made a permanent contribution to European historical writing, but this achievement was more than a matter of individual talent. By the sixteenth century the preoccupation of Florentines with their own history was already old. Ever since the great chronicle of Villani, which more than any other work created Florentine history, Florentines had built up layer upon layer of self-representation, making it possible for them to know themselves in unprecedented depth and detail. And as their knowledge of the shared past grew, so ultimately did their self-consciousness as interpreters of that past.

The classical revival of the early fifteenth century made an enormous contribution to this process. The humanists' attempt to transform the episodic narrative of the chronicles into the classical pattern of linear narrative required the highest degree of conscious literary art. Equally fruitful was their desire to generalize the Florentine experience according to patterns derived from Rome. At the same time, however, their approach to their predecessors was exclusive and disdainful, and much that was valuable was set aside. Thus it remained for later writers to make an effective marriage of the two strains in Florentine historical writing.

The signs of this reconciliation begin in the fifteenth century, but Machiavelli and Guicciardini were the first to search with conscious purpose over the whole range of texts, both vernacular and classical. Their histories that are in a strict sense the products of a historiographical tradition—works, that is, which presuppose a consciously related body of texts rather than a simple accumulation of individual accounts. Here Florentine historical writing reached its maturity, fusing the dignity, clarity, and perspective of the classical tradition with the immediacy of the vernacular; here the vernacular writer's solid sense of pragmatism and concreteness was blessed with the grace of literary form.

Reviews of Books

GENERAL

W. ARTHUR LEWIS. *The Evolution of the International Economic Order*. (The Eliot Janeway Lectures on Historical Economics in Honor of Joseph Schumpeter, 1977.) Princeton: Princeton University Press. 1978. Pp. 81. Cloth \$7.50, paper \$2.45.

This *tour de force* is based on two lectures delivered at Princeton in March 1977. In less than eighty small pages it provides an analytic history of what we now call the developing continents in relation to the advanced industrial world, an analysis of their present position, and broad policy recommendations. Compression is achieved by applying in a stylized way the conclusions W. Arthur Lewis has derived from his own study of growth under conditions of unlimited labor supply, the terms of trade, the impact of the Great Depression of the 1930s on the world economy, and the history of trade in tropical products.

He starts by explaining why Latin America, Africa, the Middle East, and Asia were latecomers to industrialization. As he sees it, the heart of the matter is that the productivity of food production per acre was about 44 percent the level of that in Europe and that these regions operated with an essentially unlimited supply of labor. The consequently low levels of real income per head inhibited the size of the domestic market for manufactures but made it attractive to produce exportable tropical products, where rates of return were higher than in basic grains. Lewis is too serious a student of growth to be content with this Ricardian comparative-cost model and adduces several supporting noneconomic forces at work, including especially the local political power of those committed to the production and export of tropical products. Although their grip was loosened earlier in some countries (for example, Mexico and southern Brazil), it was the Great Depression which broke their power and opened the way for a phase of serious, if belated, industrialization based on import substitution for consumer goods.

As Lewis contemplates the present stage of the semi-industrialized developing continents, including the pressure within them of high rates of population increase, he returns to his most fundamental proposition about modern economic growth: full-scale industrialization requires a high-productivity, modernized agriculture. He commends this path for a secondary reason: the enlargement of the domestic market a high-productivity agriculture provides permits the engine of industrial growth to be increasingly internalized, reducing dependence on the rate of growth of advanced industrial countries.

Lewis has things to say about other related matters: the causes of financial dependence; debt roll-overs; commodity agreements; and Kuznets and Kondratieff cycles. But his central theme is clearly this: "The most important item on the agenda of development is to transform the food sector, create agricultural surpluses to feed the urban population, and thereby create the domestic basis for industry and modern services. If we can make this domestic change, we shall automatically have a new international economic order" (p. 75). One must be familiar with conventional diplomatic rhetoric about the new international economic order to know how revolutionary and refreshing a doctrine this is.

There is a good deal I would add to Lewis's prescription for a new New Economic Order: notably, population, energy, and environmental policy and how to generate a serious North-South partnership on these matters and the modernization of agriculture. Moreover, I marked a number of points of disagreement with Lewis's economic history, including an outlandish account of the impact of the British monetary system on business cycles under the gold standard (pp. 47-48). And there is, I think, a printing error on the fourth line from the bottom on page 36: "low prices" should be "low wages." But I cannot recall reading more of value set out in such short compass.

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PIERRE LÉON, editor. *Histoire économique et sociale du monde*. Volume 5, *Guerres et crises, 1914-1947* by GEORGES DUPEUX *et al.* Paris: Armand Colin. 1977. Pp. 623.

PIERRE LÉON, editor. *Histoire économique et sociale du monde*. Volume 6, *Le second XX^e siècle: 1947 à nos jours* by PIERRE LÉON *et al.* Paris: Armand Colin. 1977. Pp. 607.

In 1903 Armand Colin published the twelve-volume *Histoire générale*, edited by Ernest Lavisse and Alfred Rambaud, a history predominantly concerned with the life of the state and the actions of its leaders, that is, with precisely those preoccupations that Marc Bloch and Lucien Febvre attempted to combat with the foundation of their review *Annales* in 1929. During the 1930s, the slow ascension of the *Annalistes* to intellectual dominance provoked a change in the goals and methods of much of French historical scholarship, which was consolidated under the leadership of Fernand Braudel with the seizure of academic power in the late 1940s. From the Sixth Section of the École Pratique des Hautes Études, with its vast sponsorship of dissertations and its influential publishing program, the *Annales* method was widely disseminated. The thrusts of the method were toward the "longue durée," the study of historical phenomena over long periods of time, toward the analysis of structure, especially social and mental, and toward the interaction of human beings with their physical environment. All social sciences, especially geography, economics, ethnology, and social anthropology, were to be drawn upon for help. By the 1970s the groundwork was done. The monographic infrastructure appeared sufficient. Three-quarters of a century after the publication of the *Histoire générale*, Armand Colin has brought out the six-volume *Histoire économique et sociale du monde* as a synthesis of recent historical research and especially of that influenced by the *Annales* method. Volume one, *L'ouverture du monde, XIV^e-XVI^e siècles*, and volumes five and six, listed above, appeared in 1977, to be followed in 1978 by volume two, *Les hésitations de la croissance, 1580-1730*, volume three, *Inerties et révolutions, 1730-1840*, and volume four, *La domination du capitalisme, 1840-1914*.

The series was the brainchild of Pierre Léon, former professor of social and economic history at the Sorbonne and long-time president of the French Association of Economic Historians. Léon picked the authors, planned the themes with the editors of the individual volumes, and himself edited the sixth volume on the "second" twentieth century. When Léon died in 1976, his "traité," as he called it, was already a completed legacy. The authorship of the series was unashamedly, and exclusively, French because, in Léon's words, "it is

undeniably the French school [of economic and social history] that has most perfectly achieved that tight symbiosis between the study of economic phenomena and social structures." The series, as Pierre Chaunu emphasizes in his introduction to the first volume, is "a social history which gives a broad place to economic history" completely different in character from the New Economic History favored by English-speaking historians. It is in the plan of the first four volumes, however, that the influence of *Annales* and the French concept of economic and social history is most felt. In the first volume, ethnology and anthropology are more evident than economic theory, or what Chaunu somewhat disparagingly calls "retrospective econometrics." The book breaks with the Eurocentrism of so much world history by opening with superb summaries by Bartolomé Bennassar of the economies of Black Africa before the slave trade, of the pre-Columbian American Indians, and of Asia before the Portuguese voyages. The division of the world in the fifteenth century into enclaves with little interconnection is illustrated by a study of the long-term movement of plants and animals, a form of research much favored by *Annales* historians. European society is reconstructed literally from the soil up—from exploitation of the land to family structures to urban communities to structures of thought.

With the fifth volume, which opens with the First World War, the technique of the series changes, and the *Annales* influence wanes. As Georges Dupeux writes, "with the outbreak of the First World War the time of the long-enduring ends, and the time of disruptions begins." The long term is jettisoned. The authors return to the subject so downgraded by the *Annalistes*, the "event." Unfortunately, they overemphasize the fragmentation of world economic space as the result of the Great War, and return to an excessive concentration on the European nation-state, and particularly on the difficulties of France. Japan, China, and even the European colonies are neglected in coverage. In compensation, however, René Girault's coverage of the economic aspects of the Russian Revolution and the Stalinist plans of the 1930s is succinct and useful. Political consequences of the depression in Europe and the United States are effectively tied to a study of underlying trends in employment, salaries, productivity, and regional imbalance. The social consequences of economic distress are weighed, and at times, as in the case of Weimar Germany, displaced from their accustomed role as explanation of the move to totalitarianism. Large parts of the volume are, however, unreadably encyclopedic, overburdened with random statistics, repetitive in national examples, and insufficiently synthetic.

The volume above all lacks a central theme that would have linked the study of the underdeveloped and the developed parts of the world economy.

Volume six, on the world since 1947, remedies this weakness by addressing itself to the problem of economic growth and economic backwardness. A solid foundation of analysis is provided by Jacques Magaud's survey of demographic trends. Léon himself, taking the risk of appearing occasionally trite, makes a praiseworthy effort to establish on a world scale the consequences of economic growth, such as the rural exodus made possible by the expansion of the secondary and tertiary sectors, the emergence of a managerial and technocratic elite, and the persistence of financial and social inequalities within "societies of abundance." Denis-Clair Lambert's short summary of the vicious circle of poverty in the Third World is a fine introduction to continental studies of the economic strategies employed to break out of that circle. A final analysis of the problems of the industrialized world in the 1970s lays excessive blame on the "offensive" of American industry in Europe but does acknowledge the retarding effect of unhealed social divisions, backward industrial sectors and regions, and the energy crisis.

Certain blemishes affect the whole series. The indexes are worthless. The bibliography is thin and inadequately annotated. There are no footnotes, even where issues of interpretation are being debated. More economic theory is needed. But in the study of the social effects of economic change—especially where the *Annales* method can be applied—the series provides a much needed synthesis.

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CLIVE DEWEY and A. G. HOPKINS, editors. *The Imperial Impact: Studies in the Economic History of Africa and India*. (Commonwealth Papers, number 21.) London: Athlone Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1978. Pp. x, 409. Paper \$25.00

Enthusiasm for the Commonwealth as an honorable outcome of British imperialism led to considerable academic investment in Britain after World War II in Commonwealth studies and imperial history. This investment has survived more or less conveniently to serve the interest in Third World history in the 1970s.

In 1973–74 A. G. Hopkins and Clive Dewey organized a seminar of economic historians concerned with different colonial areas in order "to explore the comparative approach as a means of linking the research of discrete groups of area specialists."

The method was "to lay specialized work on the table and hope that comparisons would emerge" (p. 6). The experience must have been invaluable for the participants, who were splendidly equipped for constructive mutual criticism. Anything achieved by way of a comparative approach, however, must have been embedded in revision of the papers before publication.

In his introduction Hopkins tries to give the book an appearance of unity by stressing the relevance of the seventeen papers to current historiographical debates, but they do not in fact contribute anything much to the general debates. They all are based on systematic use of archives, and the only unifying principle is that all of the authors, being good historians, are applying general ideas to the interpretation of particular phenomena. As discrete investigations, however, the papers are all interesting.

Hopkins's own paper, on the African origins of the Nigerian cocoa-farming industry, 1880–1920, is presented as an excerpt from a much larger study. Brief as it is, he gives almost half his space to his thoughts, which are not very interesting, about Hempel, Collingwood, and Schumpeter, but he rewards the patient reader with a fascinating glimpse into the development of modern Yoruba society. He succeeds very elegantly in arriving at one significant general proposition: "that proximity in ideology is more important than proximity in space in mapping the spread of the cocoa-farming industry throughout south-west Nigeria" (p. 94).

Dewey's article, "The End of the Imperialism of Free Trade," is of major importance. He sees the grant of fiscal autonomy to India in 1921 as a landmark that should receive as much attention as has been lavished on the turning-points of imperial policy in the nineteenth century. Any theory of economic imperialism is incomplete without an account of the mechanism by which economic interests have operated through pressure groups on administrative and political decision making. This requires analysis of the administrative and political processes and therefore of administrative and political motives and contests. It is more difficult to make plausible generalizations about these processes than about economic behavior, and Dewey's success in analyzing a measure of devolution as an incident in imperial policy should help to bring the debate about economic imperialism closer to the real world.

C. C. Wrigley, writing on "Neo-Mercantile Policies and the New Imperialism," comments judiciously on the present state of the controversy and is concerned lest the discrediting of under-consumptionist dogmas lead to a neglect of valid economic explanations. Common sense suggests that the "scramble for Africa" was motivated by a

search for resources. Hopkins, in the introduction, is unconvinced, since at the time of the scramble most tropical raw materials were in plentiful supply, but Wrigley suggests a very plausible combination of short-term political and long-term economic motives in "pegging out claims."

A second paper by Dewey, "*Patwari and Chaukidar: Subordinate Officials and the Reliability of India's Agricultural Statistics*" demonstrates the utter unreliability of the sources for George Blyn's *Agricultural Trends in India, 1891-1947*. General historians will probably go on using Blyn's figures regardless: bad statistics presented in easily usable form are irresistible. Dewey shows how the statistics could be used for local studies with proper regard for the system through which they were collected and incidentally throws light on village life in the later British period by comparing the role of the *patwari* (village accountant) in the regions of temporary and permanent revenue settlement in northern India.

David Washbrook examines the process of agrarian decision making in debt-ridden *ryotwari* villages of the "dry region" of Madras (1878-1929) in order to explain their exceptional vulnerability to famine and responsiveness to price incentives. The explanation is easy in cases where cropping decisions were taken by big men who had the power without the recognized responsibilities of landlords, but Washbrook's meticulous institutional study carefully applies the theory of dependent smallholding agriculture to recover the past as living human experience.

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DEREK H. ALDCROFT. *From Versailles to Wall Street, 1919-1929*. (History of the World Economy in the Twentieth Century, number 3.) Berkeley and Los Angeles: University of California Press. 1977. Pp. xi, 372. \$12.95.

Derek Aldcroft is undoubtedly right in saying that no single volume could provide a comprehensive account of the world economy in the 1920s. The subject is too diffuse; the monographic literature, though vast, is often pedestrian in quality; and the available statistical series contain pitfalls apt to trap those ignorant of how they were compiled. Aldcroft, a distinguished historian of British finance and macroeconomic policy, nonetheless manages to sort and summarize a substantial mass of data bearing on the international economy in the troubled post-World War I decade.

The successive volumes in the History of the World Economy series have, with one exception, attained a high standard of technical expertise.

Aldcroft's survey may lack the conceptual elegance of C. P. Kindleberger's study of the depression or the multilingual range of A. S. Milward's work on World War II; but it remains unfailingly sober in tone, dispassionate in analysis, and indefatigable in its endeavor to furnish broad thematic and geographic coverage.

Unfortunately, the book rests on no clear organizing principle. Aldcroft begins by trying to assess the impact of the Great War. Did it produce so large an exogenous shock as to make impossible the restoration of the harmoniously functioning international economy thought to characterize the second half of the nineteenth century? Or did it constitute a mere temporary diversion from long-term cyclical trends and thus have little bearing on the world economic collapse of 1929-32? The author finds the evidence contradictory, and the inconclusive result seems to have discouraged him from further efforts at bold interpretation.

Many of the decade's cross-national economic conflicts—over reparations, war debts, private lending, exchange parities, tariffs, and terms of trade—represented a continuation of war by other means; they reflected a struggle between Germany and France for Continental industrial leadership, between the United States and Great Britain for world financial dominance, between Britain and its Empire over the nature of the imperial bond, and between weakened European powers and colonial peoples that had developed productive capacity along with indigenous nationalism. In the advanced nations, moreover, state bureaucracies played a greater role in regulating the economy than they ever had before, while organized capital and labor fundamentally altered the nature of interest-group politics. Aldcroft might properly have touched on all these developments. He evidently conceived his mandate, however, to exclude consideration of diplomatic rivalries, domestic social tensions, and institutional transformations that helped shape the world economy. He avoids generalizations and concentrates on elucidating purely economic trends.

Within this circumscribed framework Aldcroft displays considerable virtuosity. His analysis of the boom-and-slump cycle of 1919-21 is masterful, and his account of reconstruction in 1921-25 does much to explain why Europe's relative economic decline became permanent. He offers an illuminating summary of the literature on what went wrong with the gold exchange standard and shows convincingly why Britain's export position would not have improved greatly if it had stabilized in 1925 at a lower parity. He also presents a judicious outline of the debate whether monetary policy errors or "real" forces caused the Great Depression. All too often, however, this book reads like one of those

descriptive League of Nations monographs from which the author drew his source material. This country benefited from increasing demand for its exports; that one suffered from insufficient demand elasticity for its single primary product; a third went through a difficult patch when synthetics crowded its traditional markets; yet a fourth relied on handicrafts and continued to stagnate. The statistics mount inexorably. Every contributor to the *Economic History Review* receives his due, every specialist on copper or rubber an appreciative citation (though Elmus Wicker may not be pleased to see his book on the Federal Reserve attributed to E. R. Walker). The point frequently remains obscure.

In short, Aldcroft has written a useful manual. Every specialist on interwar history will want to have it in his reference library. Only the most resourceful will succeed in persuading students to read it from cover to cover.

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JOSÉ MARÍA RIPALDA. *The Divided Nation. The Roots of a Bourgeois Thinker: G. W. F. Hegel*. Translated by FAY FRANKLIN and MARUJA TILLMANN. (Dialectic and Society, number 3.) Assen: Van Gorcum; distributed by Humanities Press, Atlantic Highlands, N.J. 1977. Pp. vii, 221. \$24.75.

GEORGE ARMSTRONG KELLY. *Hegel's Retreat from Eleusis: Studies in Political Thought*. Princeton: Princeton University Press. 1978. Pp. ix, 259. \$16.50.

Both of these books take as their underlying premise that Hegel's philosophy attempted to resolve one of the problems that was first raised during the French Enlightenment in the *Querelle des anciens et des modernes* and that still bedeviled German writers at the end of the eighteenth century. That is, if it was impossible to reconstruct the harmony of politics, culture, and ethics that characterized ancient Greek civilization, how was the modern world to overcome the division that existed between traditional political and ethical values and the harsher realities of modern social and political life? It is this division between the *ought* and the *is* that both authors see Hegel attempting to resolve. For both, he represents a transition figure between the empiricism of the Enlightenment and a modern dialectical analysis of history and philosophy.

George Armstrong Kelly treats Hegel as primarily a political philosopher who proposed a means by which the ideals of the *Rechtsstaat* might be achieved in a "Neutral State." This Neutral State would be "realist in power, idealist in purpose" (p.

136), a state which combined strong authority with respect for the intellectual freedom of the middle class. Only by this means could the state assure itself of access to the kind of precise knowledge and objective truth that an enlightened regime required. For Hegel, the ultimate guarantors of this wisdom should be a mandarin intelligentsia employed in the civil service. Their most formidable antagonists, he felt, were churches, institutions of tyranny, and prejudice. With all its limitations, Kelly suggests, Hegel's ideal of a Neutral State remains a credible and viable political ideal for post-Watergate America and the West.

In contrast, José María Ripalda devotes almost no attention to Hegel's political philosophy as such, particularly as it was expressed in the *Philosophy of Right*. Instead, his book offers an extensive resumé of Hegel's relationship to the poets and philosophers of his generation and focuses on Hegel's youthful writing as an analysis of the division between political ideals and social reality articulated in the *Querelle*.

The starting point of Hegel's philosophy, he feels, was his ideological reflection of social divisions in his own day. From the time Hegel came to recognize that the old Greek utopia could not be regained because "it was inferior to the state of reflection brought by Christianity," Ripalda claims, his life's work was an effort to "reach an understanding of Capital, to analyze the modern division in terms of the social and economic conditions of the world after Adam Smith" (p. 163). Though youthful writings like *Concerning Some Characteristic Differences Between the Ancient [and Modern] Poets* (1788) hint at the solution, they mistake the role of the proletariat. It would take half a century "for the logic of the Concept to be substituted for the logic of Capital" (p. 133).

Both books are flawed by the authors' frequent digressions into the works of recent students of Hegel, his debts to obscure contemporaries, and the projection of current political or social problems into Hegel's work. If Ripalda may be faulted for his determination to see Hegel through a filter of modern Marxism, Kelly's Hegel seems still more curious when converted to a commentator on post-Watergate political theory.

Most disappointing are the books' blindspots. How can Ripalda write a book that so completely ignores Hegel's enormous labor in constructing a philosophical system and pretend that his writings were chiefly concerned with capital or the liberating role of ideology? In one of his finest chapters, "Lordship and Bondage," Kelly challenges this tendency of Marxists to ignore the purely philosophical interests of Hegel. He also suggests that in seizing upon the master-slave dialectic as an anticipation of Marx, these writers overlook He-

gel's relegation of that contest to but one stage of the development of consciousness and his belief that both elements were equally important to the progress of the spirit.

Still, Kelly has his own blindspot, and quite ignores Hegel's disregard of such primary components of Prussian life as militarism or the political power of a fossilized aristocracy. It was that aristocracy, after all, not the intelligentsia, who were the dominant element of the Prussian bureaucracy. Is this the political pattern Kelly would offer as a model to Western states that today face their own problems of militarism, technocratic government, and the establishment of a more democratic direction of industrial policy?

Unfortunately, Ripalda's book abounds with errors of grammar and spelling. This is nowhere more harmful to the force of his argument than in the final pages, where a cryptic style, uncertain grammar, and, perhaps, some missing words render much of the text incoherent.

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ROMAN ROSDOLSKY. *The Making of Marx's 'Capital'*. Translated by PETE BURGESS. London: Pluto Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1977. Pp. xvi, 581. \$35.00.

This book represents one man's attempt to rescue the Marxist tradition from the "unparalleled degradation and sterility" it suffered during the Stalin era and to further the development of a vibrant, critical (that is, dialectical) Marxism that, according to the author, reached its "relative high-point" in the 1920s (p. 570). Roman Rosdolsky's inspiration derives from his own tragic experience as a Marxist intellectual. In the 1920s, he worked at the Marx-Engels Institute in Moscow, but in the 1930s had to flee the Stalin purges only to wind up in one of Hitler's concentration camps until the end of World War II. He then moved to Detroit, where, in the 1950s and early 1960s, he wrote this book, which first appeared in German in 1968 and which, a few years after Rosdolsky's death, has finally been translated into English.

The primary aim of the book is to demonstrate the fundamental role in Karl Marx's *Capital* of dialectics as a method of social scientific investigation. To do this, Rosdolsky draws upon Marx's extensive notes on political economy written in 1857-58 as part of the process of writing *Capital*. These notes, now known as the *Grundrisse*, were published in German in 1953 and in English in 1973, but when Rosdolsky began his book in 1948, their existence, not to speak of their contents, were known only to a small number of Marxist scholars.

The main virtue of the *Grundrisse* is that Marx's dialectical methodology is much more explicit there than in his more complete theoretical analysis in *Capital*. Thus, according to Rosdolsky, the *Grundrisse* "takes us into Marx's scientific workshop, and allows us to witness the process by which his economic theory develops" (pp. 210-11). Moreover, "without the appropriation and thorough assimilation of the methodological discoveries of *Rough Draft* [the *Grundrisse*], it is impossible . . . to make any real progress in the field of marxist economics" (p. 570).

In *The Making of Marx's 'Capital'*, Rosdolsky first uses the *Grundrisse*, along with other relevant documents, to explain why Marx changed the initial outline for his work on political economy and why he ended up structuring the three volumes of *Capital* the way he did. Then Rosdolsky skillfully integrates the material from the *Grundrisse* into a methodological and theoretical interpretation of the actual contents of *Capital*. After a brief discussion of the "law of value under socialism," the author brings his own interpretation of Marx's political economy into the twentieth century by means of a critical evaluation of reproduction and realization theories by Marxists such as Tugan-Baranovsky, Lenin, Hilferding, and Luxemburg. He then attacks Joan Robinson's critique of Marx's economics, arguing essentially that she is ignorant of Marx's method. Finally, he examines the textbook on political economy by the Polish Marxist, Oskar Lange (first published in its unfinished state in 1959), finding that it "contains nothing, or almost nothing, on the method of Marx's *Capital*" (p. 568), despite the important contributions on this subject made in the 1920s by Marxist intellectuals such as Lukaács, Preobrazhensky, and Rubin—and despite the surfacing of the *Grundrisse* in the 1940s. Rosdolsky's obvious point in concluding his book on this note is that (writing in the 1960s) the methodology of Marx still needs to be imparted to Marxists.

Rosdolsky's book is certainly important in this regard, although, with the diverse and self-critical development of Marxist social science in the 1970s, his is no longer by any means a voice in the wilderness. *The Making of Marx's 'Capital'* will be especially useful to students of Marxian and post-Marxian economic theory because of its integration of the material in the *Grundrisse* and *Capital* and because of its excellent analysis of Marxist political economy in the first three decades of the twentieth century. Although many of Rosdolsky's views are controversial—for example, his definitions of the dialectic ("content and form in constant interaction with one another" [p. 564]) and his analyses of underconsumption and the falling rate of profit in capitalist development—he treats

the issues of Marx's methodology and theory, as well as a wide range of post-Marxian developments in political economy, in a clear and intelligent manner.

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JERROLD SEIGEL. *Marx's Fate: The Shape of a Life*. Princeton: Princeton University Press. 1978. Pp. ix, 451. \$16.50.

Reviewers of *Marx's Fate* have been misled by its introduction to consider it a study in psycho-biography. According to their proclivities, many reviewers have on this basis dismissed the book out of hand. Jerrold Seigel encourages such reactions by characterizing his treatment of Marx as a "psychological perspective." What he has given us is, rather, a full intellectual biography of Marx, integrating exhaustively his life and politics with an analysis of his writings.

As an intellectual biography the book is impressively researched, fair in its judgments, and written in sober and clear prose. Seigel's scholarly good sense allows him to present a successful portrait of one of the most controversial figures in European intellectual history, indeed in world history. Along with the work of David McClellan, *Marx's Fate* ranks as one of the solid accomplishments of recent Marxography.

Seigel is especially successful in exploring obscure intellectual filiations and in inventing illuminating comparisons. He presents fascinating discussions of the concepts of life cycle in Hegel and Max Stirner. Suggestive also is his comparison of Urquhart, a cranky British politician, with the character of Rameau in Diderot's famous dialogue. All in all, Seigel has mastered well an impressive body of primary and secondary literature and has synthesized it into a compelling study of Marx.

The serious flaws in the book derive from Seigel's unrepentent historicism and from his timidity in applying the instruments of psychology in his task. Hampered by the naive epistemological premises of historicism, Seigel employs only those psychological categories actually available to Marx. Since it is Seigel, not Marx, who is undertaking the study, he should have utilized those categories available to him, not those available to Marx. Seigel writes that "we do not need to move outside the context" of Marx's life to analyze it (p. 7). Unfortunately for Seigel, he is not a contemporary of Marx and is situated securely enough in the twentieth, not the nineteenth, century.

That simple exigency betrays its mark as Seigel confusedly acknowledges his debt to Erik Erikson, one who is surely "outside the context" of Marx's

life. Seigel's Eriksonianism leads him to give an emphasis to the role of childhood experience in Marx's life that Marx himself might not accept. That, of course, is proper, but it does not jibe with historicism. The result of Seigel's ambivalent allegiance to psychohistory is an unsystematic, impressionistic exploration of the relation of Marx's character to his work. In a brief, very brief, concluding section, Seigel sums up the life of Marx as a conflict between internal and external reality. Although Seigel's portrait of Marx is both sensitive and sensible, it does not take us very far in comprehending the psychic roots of historical materialism. A full psychohistorical study of Marx remains to be written.

Nevertheless, *Marx's Fate* is a valuable contribution to the intellectual history of the nineteenth century. Seigel takes the reader with a sure hand through the entire corpus of Marx's writings, from the high school essays to *Capital* itself. Although not everyone will agree with Seigel's emphasis—there is very little discussion of historical materialism and its adequacy as a theory—most will be pleased by his grasp of the ideas. By the same token, Seigel bases his discussions on a wide reading of the commentaries on Marx, although there are some serious omissions such as Roman Rosdolsky's impressive study of *Capital*. In the end, *Marx's Fate* can hold its own with McClellan's biography even though I suspect most readers will give the edge to the latter.

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WILLIAM H. SHAW. *Marx's Theory of History*. Stanford, Calif.: Stanford University Press. 1978. Pp. 202. \$12.50.

It is most comforting to have the scheme with which one grew up confirmed by sophisticated analysis. What my generation learned in school about Marx's interpretation of history was a neat and systematic hierarchy, self-explanatory and intellectually satisfying, and one, therefore, that we treasured. According to it, the forces of production, continuously developing, constituted the basis and driving power of all history. It determined the relations of production and the mode of production that was spawned by those relations, the structure of society, the forms of government, and the dominant ideas of the culture. In short, the economic substructure determined the social, political, and ideological superstructure. This scheme, hollow though it may have been, was supported by Marx's summary statement on historical materialism in the preface to his *Contribution to the Critique of Political Economy* (1859).

One might think that a book like William H. Shaw's *Marx's Theory of History*, written in defense of this familiar thesis, would be neither interesting nor challenging, but this would be wrong. There are three reasons why Shaw's treatment, based on evidence collected carefully from all of Marx's and Engels's writings, amounts to far more than a cogent argument in favor of a traditional interpretation.

First, Shaw disarmingly limits his focus to the empirical side of Marx's historical theory as Marx himself conceived it, and he thereby excludes from consideration both the later Marxist elaborations and the direct philosophical analyses of what Marx really meant, over and above what he said he meant. By virtue of this disclaimer and the corresponding claim that Marx's own concept of historical materialism has not heretofore been impartially examined, Shaw not only faithfully communicates his intentions, but also makes this book a contribution to Marxian studies by using current scholarly methods to support a no longer current advocacy of Marx's overall consistency. This contribution is valuable, although Shaw's disclaimer is somewhat deceptive (as we shall see).

Second, Shaw does more than merely argue for the terms of the traditional theory—he fleshes them out in ways that are undoubtedly Marx's own but that are not apparent in Marx's usage. Productive forces thus include both means of production (themselves broken down into both the instruments and the objects of labor) and labor power. Shaw also distinguishes, within the relations of production, between work relations, which are independent of any specific social form, and ownership relations, which have a definite socially determined form. This distinction is indispensable for understanding how the rationale of revolutionary change works in Marx.

Third, Shaw shows a fine analytical discrimination in separating what his view of Marx's theory accounts for from what it does not. Thus, he demonstrates that the "technological determinism" that he attributes to Marx means only the general determination of history by the forces of production and leaves the actual forms of classes, institutions, and ideas open to the free play of the particular societies. Related to this sophisticated outlook is the acknowledgment that "materialism" for Marx was a broad-gauged concept including mental factors, and that Marx's economic determinism was generously rather than narrowly thought out and included a large measure of cultural interaction within the large limits set by economic necessities. Shaw does not hesitate, moreover, to point out the areas in which Marx's thought fails of its usual consistency. Especially striking in this respect is Shaw's analysis of the discrepancy, within Marx's general historical determinism, be-

tween the indeterminism of precapitalistic societies and the inevitability of capitalistic society's transition to socialism.

The one problem with the book—and it is an important one—is whether its achievements are really grounded in Shaw's scholarly objectivity and philosophical self-limitation or whether he is moved by his own principles of Marxian interpretation. To this reviewer the second of these alternatives holds. In his introductory statement Shaw lists both his intention to study Marx in Marx's own terms and his own commitment to the "technological-deterministic interpretation" of Marx. Not only are the two assumptions different in kind, since one is methodological and the other substantive, but the second overshadows the first. Although Shaw nominally abjures questions of Marx's intellectual development and of the Marx-Engels relationship, he actually takes a position on these issues by drawing his material from all stages of Marx's career (starting with the *German Ideology* of 1845) and selecting from Engels only those items that support his argument. A characteristic example, which indicates both Shaw's scholarly ingenuity and his limits in this respect, appears in his argument that "there is nothing particularly Oriental" in Marx's version of Asian primitive communism: he adduces as evidence for this argument the translation of Marx's "das ursprüngliche orientalische Gemeineigentum" into the English "the primitive form of ownership in common," an omission of the specific oriental motif duly noted to have been "supervised by Engels" (p. 126). Persuasive perhaps, but for Marx the opposite point could be made from the same evidence.

Shaw has written a fine book defending the traditional economistic and deterministic Marx in a scholarly way on the most favorable possible terrain. It is a defense which has the incontestable merit of making Marx's economics intellectually accessible and the contestable demerit of asserting the possibility of understanding Marx without any philosophical presuppositions, ruling out of court as interpolative other interpretations of Marx that have such presuppositions, whether of the structural or the humanistic variety. But a total thinker like Marx requires a correspondingly total approach. Shaw violates his own self-denying ordinance because to do otherwise is simply not possible.

LEONARD KRIEGER
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BARUCH KNEI-PAZ. *The Social and Political Thought of Leon Trotsky*. New York: Clarendon Press, of Oxford University Press. 1978. Pp. xx, 629. \$34.95.

Baruch Knei-Paz defines his task in this book in a highly specific way: he wishes to give an account of

Trotsky's theoretical views without, so far as possible, giving any account of Trotsky's deeds or of the views of those others, such as Lenin, in debate with whom Trotsky formulated his own theories. Moreover, he explains Trotsky's thoughts topic by topic rather than in the order in which they occurred, although the topics themselves are arranged in the order in which they become important preoccupations of Trotsky's. He has, in addition, largely avoided controversy with rival interpretations of Trotsky's thought, such as that advanced by Isaac Deutscher. Castigating other studies as not being "comprehensive and free of partisanship" (p. x), Knei-Paz aims to provide as complete an exposition as possible and a critical discussion.

The first part is concerned with Trotsky's explanations of the sociology and politics of backwardness and particularly with his account of those phenomena that he labeled "combined and uneven development." In the second part, Knei-Paz develops Trotsky's views of political and social revolution. In the third part, he examines Trotsky's criticisms both of Stalin's doctrine of "Socialism in One Country" and of the actual development of the Soviet state. The fourth part is reserved for Trotsky's views of art, literature, philosophy, science, history, biography, and the Jewish question.

There is something very curious about the whole project. Since the majority of Knei-Paz's six hundred pages is taken up with quite lengthy summaries of well-known works, why read Knei-Paz rather than the originals, more particularly since the leaden academicism of Knei-Paz's style compares very badly with Trotsky's vivid prose? Moreover, as Knei-Paz admits, Trotsky's themes were always formulated and reformulated in response to social and political events and in the context of controversy with others, so the attempt to present them in abstraction from this context necessarily leads to the writing of what is essentially a truncated and deformed history. What Knei-Paz has in fact done is to present Trotsky's theories in much the same mode as modern academic political scientists and sociologists present their theories for evaluation by their colleagues. Presumably, this procedure might be justified if Trotsky's theories were then brought to the test of illuminating empirical and historical materials and compared with its rivals in this respect. But this does not happen either. Knei-Paz makes some useful beginnings, perhaps most of all in his discussion of backwardness, in which he accuses Trotsky of not having appreciated the difference between industrialization and modernization. But even here what is said is far too brief.

Knei-Paz himself says that, although "this book

may be described as a work in the field of what is sometimes called the history of ideas, it is not in itself a work of history" (p. xii). What has to be learned from this book is that there is no way to do the history of ideas adequately without doing genuine history and, more particularly, that the treatment of social and political ideas in abstraction from those contexts from which they derive their life is doomed to failure, something which Trotsky knew very well, but which Knei-Paz failed to learn from him.

ALASDAIR MACINTYRE
Boston University

CHARLES TILLY. *From Mobilization to Revolution*. Reading, Mass.: Addison-Wesley. 1978. Pp. xiii, 349. \$7.95.

Essentially a practitioner's handbook for the study and analysis of collective protest, this study has something of a grab-bag quality. The title to the contrary, it deals with far more than revolution, though one of the most successful chapters offers an assessment of the state of current thinking on how to define revolutionary situations and outcomes. At times, however, the book drifts into general comments on collective action, not protest alone. Indeed, a disconcerting number of illustrations of the workings of organizations are drawn from recent American organizational phenomena that have little to do with protest, presumably to help student readers with their identifications.

But the book is mostly about protest: how to define it, how to measure and schematize it, and how to recognize major interpretive schools. No overarching theories of protest are offered, but Charles Tilly vigorously pushes his previous analysis of the power struggle and rational purposiveness involved in protest action, and in subsections sums up earlier work on the main lines of the evolution of strikes. The historical frame of reference is mainly West European, though the section on revolutions ranges further. The bulk of the analysis assessed and used is in the tradition of American social science, after a chapter on the heritage of Marx, Weber, and Durkheim. Specific historians' findings are selectively employed and a general bow is made to West European Marxist writings. Tilly does not aim to offer new factual material, though the text and appendixes suggest the ongoing work of his research group on contentious gatherings in Britain.

The book seems intended mainly for advanced seminars in political sociology. Two large chapters present highly generalized schemas on protest mobilization and opportunity/repression, replete with diagrams. The analysis, immensely well in-

formed, rests mainly on sophisticated common sense, typically phrased in terms of cost-benefit formulas whereby protesters weigh risks against available resources and anticipated gains. Attention to definitions (what violence is, the difference between protest as explained by external causes and protest as resulting from individual or group purpose) can be somewhat wearing, but a number of important problems are clarified. The predominant interest lies in measurable or potentially measurable attributes of collective action, relating particularly to organizational levels and the attributes of the state-as-opponent. Correspondingly the best chapter with significant historical content deals with changing repertoires of protest forms. But the purposes of protest—in terms of the competitive, reactive, and proactive labels used in the author's earlier work—are dealt with hurriedly.

There are, inevitably, huge gaps. Despite Tilly's interest in protest as a political phenomenon, the most directly political forms of collective action, apart from revolution, are ignored; again, there is no sense of a general historical theory beyond protest mechanisms. The discussion of repression, so useful as a topic, wanders into definitional problems concerning the effective polity, and nongovernmental entities are not well covered. Attacks on Durkheimians and a number of American political scientists, aimed particularly at their analysis of mass anomie or deprivation as effective protest motors, are well taken, but problems with the author's own schema—particularly the claim that all protest issues boil down to a contest for power and his consistent discounting of emotion—might have been given more attention, if only to create a sense of open-endedness then to be attacked once more. Indeed, the author tends to build on previous statements that were more carefully balanced, as if time, without additional data, had transformed them into incontrovertible fact.

A host of insights remain about what protest is and has been and how it can be studied. Beyond its substance, the book has interest for more humanistic historians as an example of how a good historian/sociologist addresses the social scientists among his constituency. But here, a final disappointment must be registered. The book ends with a plea for the relevance of the history of protest, but it is somewhat lame, trailing off with another account of the author's general work on France. Save for some aspects of the definitional formulas, which have, among other things, relevance for the handling of sources (an issue which the author otherwise treats sparingly, perhaps from a fear of overburdening his model-minded audience), there are few interesting historical problems presented. Periodization, most notably, does not figure prominently—the most general schema are meant to be

time-free—and when it does it is presented as a datum, not an ongoing analytical opportunity. And except for the chapter on protest repertoires, the vividness of past protest, or of historians' efforts to interpret it, does not emerge either. Tilly promises us further thinking on the history of collective action and on interpretive models. One looks forward to the results—assuming that they will significantly amplify the generalizations repeated here—more on the basis of previous work than on reading this strangely dry interim report.

PETER N. STEARNS
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NIKOLAI N. BOLKHOVITINOV. *Russia and the American Revolution*. Translated and edited by C. JAY SMITH. Tallahassee, Fla.: Diplomatic Press. 1976. Pp. xiv, 277. \$29.70.

Nikolai N. Bolkhovitinov is probably the most distinguished Soviet historian of the United States—it is a trade that does not flourish there. The present book is a translation of *Rossia i voina SShA za nezavisimost', 1775-1783* (1976), which is a somewhat expanded part of his previous book, *Stanovlenie russko-amerikanskikh otnoshenii, 1775-1815* (1966), published recently by Harvard University Press as *The Beginnings of Russian-American Relations, 1775-1815*, translated by Elena Levin (1975). The fresh material in the new book is marginal at best.

Several features of the translation are worthy of note. First, the translator worked from the Russian manuscript furnished by the author rather than from the book. Perhaps for this reason, bits of the published Russian text do not appear in the translation. The reasons for this procedure are not explained, and the virtues of it are not self-evident. Second, the system of transliteration of Russian is neither that of the Library of Congress nor of the New York Public Library, and is perhaps not a system at all. It is consistent only in its awkwardness. Third, the quality of the translation ranges from the imprecise and inelegant to the inaccurate and incomprehensible. Some of the stylistic peculiarity is a faithful reflection of the author's own language, but the translator has embellished the work with additional misfortunes. He has introduced errors of fact, for instance by making mistakes in proper names which the author had used correctly. He has altered meanings of the original, for example, he renders *davno* as "recently" rather than "long ago," and *uzhe ne* as "already not" rather than "no longer." He has translated back into English Russian translations of English sources rather than quoting from the sources themselves. And he has written English sentences such as: "It had scarcely just become

known in London in the spring of 1781 about the appointment of Francis Dana to St. Petersburg before the British Foreign Secretary, Lord Stormont, was not slow in advising Russian Minister Simolin that in England 'they are not at all reassured by this, and think that they would give offense to the friendly feelings of Her Imperial Majesty for Great Britain if they suspected even the slightest desire on her part to receive this new Minister at her Court' " (p. 66).

The quality of the content of the book is also questionable. There is some information of interest here, particularly about diplomacy, Russo-American trade, and the scientific correspondence among various Russians and Americans, but the factual data are not always accurate. (James Duane is referred to as John Duane, Rufus King as Robert King, Robert R. Livingston as Robert A. Livingstone.) More importantly, conclusions drawn from the data are often of doubtful validity. For example, Bolkhovitinov maintains that the 1780 League of Armed Neutrality and Russian efforts to mediate diplomatically in the American Revolutionary War had a significant effect on the outcome of the war, and yet in a different context he indicates that until the very eve of Yorktown the British adamantly refused even to consider a negotiated peace. On the whole, the Levin translation of Bolkhovitinov's earlier work is considerably more useful, and better still are the various works of David Griffiths and Max Laserson.

But if the book is of limited value as history, it is extremely interesting as an example of Soviet scholarship in the service of politics. It is a veritable paean to detente. Its message is that "the history of Russo-American relations . . . shows that in the most difficult critical periods of the history of the United States—during the War of the Revolution, 1775–1783, the Anglo-American War of 1812, and finally, the Civil War of 1861–1865—the position of Russia . . . turned out to be objectively benevolent." Upon those early foundations, the two countries "accumulated a great and varied experience of fruitful scientific, cultural, social-political, and trade ties. The substantial turning point in the relations between the USSR and the United States which has taken place in recent years makes it possible to hope that these traditions will experience further development, to the benefit of our peoples and of all mankind" (p. 188). Obviously, such a prospect has for the moment passed, and it is to be hoped that, should it recur, the practitioners of detente in Moscow—and in Washington—will give us a better example both of form and of substance than this one.

HUGH RAGSDALE
FORREST McDONALD
University of Alabama

HANS-ULRICH THAMER and WOLFGANG WIPPERMANN. *Faschistische und neofaschistische Bewegungen: Probleme empirischer Faschismusforschung*. (Erträge der Forschung, number 72.) Darmstadt: Wissenschaftliche Buchgesellschaft. 1977. Pp. xiii, 268.

Faschistische und neofaschistische Bewegungen, one of a growing number of efforts to systematize our understanding of a protean fascist phenomenon, is an attempt to categorize fascist movements in Spain, Argentina, Southeastern Europe, France, and Italy. The authors also propose in their subtitle, *Probleme empirischer Faschismusforschung*, and elsewhere in their book an empirical testing of currently accepted theoretical analyses of fascism. Hans-Ulrich Thamer and Wolfgang Wippermann, however, have written less an empirical study of fascism than a historiographical essay, as is evident from the impressive collection of footnotes. No new evidence directly from the fascist movements is introduced to help create a new typology, which the authors propose. Nor are directions for new research suggested.

In their examination of theories of fascism, the authors focus mainly on Marxism, which they strongly criticize, and modernization theory, to which they appear ambivalent. They contend that a typology of fascism is required prior to the successful elaboration of a global modernization theory, to which, however, they concede some utility in particular cases. In relation to France they suggest that fascism in general was "einer vor allem sozialpsychologischen Krise beim Übergang zur industriellen Massengesellschaft" (p. 155). Unfortunately, the field of psychoanalytic history, alluded to in the foregoing quotation, is explored only briefly in a footnote (p. 10, n. 24).

The issue of modernization versus reaction, the Janus face of fascism, to use Alan Cassels's term, is also left unresolved by the authors. They emphasize the continued industrial modernization in Germany after 1933, reiterating the point in reference to Peronism. In the conclusion, however, they take a more balanced view, recalling some of the more retrograde characteristics of fascist movements, National Socialism in particular (pp. 243–44).

The authors are more incisive in their opposition to Marxist interpretations, but they also reject all attempts to understand fascism through study of its social support, arguing instead that "die Klärung des Problems der sozialen Basis wenig über das Wesen des einzelnen Faschisten aussagt" (p. 240). They also condemn the idea of social function, preferring instead the concept of *Zielsetzung*, or goal setting, which does not tell us who sets the goals, or why, or anything of conflicting goals within particular fascist movements. Social analy-

ses such as those of Peter Merkl and Juan Linz offer more here.

The authors' disavowal of social analyses of fascist elites and their followers leads to several problems. The Arrow Cross movement and Italian Fascism are both described as having histories independent of social classes, leaving the authors uncertain as to the social function of the former, with the latter seen as largely independent of the Italian business community. Not a tool of capitalism, the Italian party provided an authoritarian substitute for a deficient capitalist organization in Italy. Perhaps, but does this negate the importance of those business interests that supported Mussolini, especially after 1922?

The typology the authors seek does not succeed: it provides no clear guidelines for the classification of Franco's Spain. The authors also assert that the virulence of fascist hatred was more significant than the identities of their targets, whereas the roster of enemies, in this case all with internationalist associations, may tell much about political movements.

Thamer and Wippermann have written a good scholarly survey of several fascist movements and some of the problems incurred in trying to classify them and arrive at a coherent definition of fascism. The limitations of their book, however, will restrict its influence in the continuing and often vexing discussion of the meaning of fascism.

BERTRAM M. GORDON
Mills College

RENZO DE FELICE. *Interpretations of Fascism*. Translated by BRENDA HUFF EVERETT. Cambridge: Harvard University Press. 1977. Pp. xvi, 248. \$15.00.

We are fortunate to have this cogent selection of "interpretations of fascism" available in English. Renzo de Felice mentions most of the significant ones from the early 1920s to the late 1960s. The first part of the book deals with interpretations of the fascist phenomenon, the second part with Italian interpretations of Italian Fascism. Although many of these selections are available in other anthologies, the scope of this collection is broader than most—from the first Marxist reactions to the "March on Rome" (including a 1923 essay in German by the Hungarian Djula Šaš) to recent modifications of modernization theory as it pertains to fascism. But, as Charles F. Delzell says in his introduction, there are some surprising omissions. Since this volume is a translation of the 1972 Italian edition, it does not include recent books on Italy by Roland Sarti and Adrian Lyttelton. But there should have been some discussion of pre-1971 works on the Nazi regime by Karl Dietrich Bra-

cher, on the rise of Italian Fascism by Roberto Vivarelli, and on the Marxist interpretation of fascism by Nicos Poulantzas.

In the conclusion to this work De Felice offers for the first time his own tentative interpretation of the fascist phenomenon. He later developed this further in *Fascism: An Informal Introduction to Its Theory and Practice* (an interview with Michael A. Ledeen [1976]), and he now has in press a section on fascism in the Italian *Encyclopedia* covering the twentieth century. In the earlier volumes of his biography of Mussolini, De Felice had agreed with most other commentators that the fascist seizure of power relied heavily on the support of lower-middle-class Italians who feared displacement and proletarianization during the immediate postwar crisis. Now he contends that the modern sections of the lower-middle-class wanted to put themselves forward as a new, "emerging" class: "These elements asserted themselves as a class seeking to gain power and to assert its own function, its own culture, and its own political power against both the bourgeoisie and the proletariat. To put the matter briefly: They wanted a revolution" (*Fascism: An Informal Introduction*, pp. 47-48). In the volume under review De Felice also asserts this interpretation: "Even the middle classes aspired to their own revolution; in Italy, they thought they could attain it by means of Fascism" (p. 185). He claims to have precise documentation to verify this contention, but until he publishes it we have only another hypothesis, albeit a tantalizing one.

De Felice knows more about Italian Fascism than any other historian, but he is unsure of himself when he tries to elucidate what he knows. (Not incidentally, he misuses "interpretation," which has to do with meaning, in discussing writers who are primarily concerned with causal explanation.) Theory is not his forte. He remarks, somewhat grudgingly, that "we must acknowledge the significant contributions made by social scientists to our understanding of fascism," but he barely conceals his hostility to "the theories, hypotheses, and methodologies of the social sciences" (p. 77). Yet he blithely speaks of "mass society," "the secondary mobilization of the middle classes," and "typically petty bourgeois myths," as if these theoretical constructs were empirical facts.

De Felice sees himself as a historicist: the essence of fascism is the history of its individual national manifestations. And, since he feels that there were major differences between the only two bonafide fascist regimes (the Italian and the German), he ought logically to conclude that fascism as a generic term is useless in trying to grasp the once-living realities of those two regimes. But, taking his cue from Angelo Tasca, De Felice rejects this conclusion as too extreme. He insists that we

not "obliterate . . . the notion of Fascism as a phenomenon . . . [lest we] lose sight of that modicum of common denominator to which I have referred" (pp. 174-75). The first common denominator he notes is geographical and chronological: "Fascism was a European phenomenon that developed within the time span encompassed by the two World Wars" (p. 175); it was a response to the economic, political, structural, and sociopsychological crises of that era. The second is that the middle-class was the social base of fascism. Third, fascism was totalitarian, and, fourth, it was revolutionary.

This, then, is De Felice's "balance sheet" of the "interpretations" he discusses in this book. It is a compromise between theories of the middle range and those that emphasize the unique historical processes in individual countries. It is also an amalgam of specific points from contrasting explanations. De Felice finds the Marxist class analysis too simple, but he does not deny that "fascism must be viewed as a manifestation of class struggle" (p. 186). The only explanation for the rise of Italian Fascism he dismisses out of hand is that of his archrival Denis Mack Smith, whose explanation, ironically, is more historicist than his own.

EDWARD R. TANNENBAUM
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SOILY ZUCKERMAN. *From Apes to Warlords*. New York: Harper and Row. 1978. Pp. xv, 447. \$20.95.

This memoir is highly recommended to young scholars. In it the author tells how he started out to be a medical man, became interested in apes, performed a variety of scientific experiments, published, made a name for himself, and, when a national crisis came, sought to make a place for scientific enquiry in the war effort. He was able to do all that while continuing to keep up in his own special field, anatomy, and gradually becoming a scientific adviser to the government where he worked to save lives by finding the most efficient way to conduct man's largest group activity, war.

This first volume carries Solly Zuckerman's story only to the end of World War II, though he had a distinguished career in science and defense for another thirty years thereafter. In the vital six years from 1939 to 1945 he became intimately acquainted with most of the top airmen in the Anglo-American air forces in Europe. Since he was considerably younger than most of them, he has had the benefit of writing after almost all of the official histories and private memoirs have appeared. The result is a mass of material that amplifies, evaluates, corrects, and sometimes merely asserts what

went on in the corridors, offices, and tents of power.

Zuckerman was born and raised in South Africa, qualified as an M.D. in London, and then joined the staff of the Zoological Gardens as a research anatomist. Soon he received an offer from Yale, then a Rockefeller Foundation grant, and finally a post at Oxford, where he dined with Professor (later Lord) Cherwell, Churchill's right-hand man in World War II. He also married the daughter of the Marquis of Reading. Once the war started, he began the Oxford Extramural Unit, which engaged in the scientific examination of phenomena—including the German bombing of Hull and Birmingham—in order to get empirical data from which to calculate what could and could not be done.

One thing led to another and Zuckerman soon found himself in the Middle East, where his work on bombing analysis brought him to Tedder's attention. Apart from the examination of Tripoli, his two most important works in the Mediterranean theater were the calculations used as the basis for the aerial neutralization of Pantelleria and the study of the breakdown of transportation in Sicily. The latter was particularly important since it became the basis for the transportation plan implemented so successfully before and after D-Day in 1944. A large part of the book is devoted to the struggles against the "bomber barons" (Harris, Spaatz, and their staffs) to get the plan into action, to his description of the problems of setting up the evaluation teams, and to the strange way in which the German corroboration of his arguments came into his hands and was then suppressed. Finally he presents the background not told by Webster and Frankland of the British Bombing Survey Unit, whose volumes have still been denied publication (in sharp contrast to the many slim studies by the USSBS).

Zuckerman ends with an analysis of the leaders he knew and with rightfully high praise for his friend, that intellectual Marshal of the RAF, Lord Tedder.

ROBIN HIGHAM
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LEILA J. RUPP. *Mobilizing Women for War: German and American Propaganda, 1939-1945*. Princeton: Princeton University Press. 1978. Pp. xii, 243. \$12.50.

The first historians who sought to interpret the relationship between war and women's status depicted World War II as a watershed for American women. More recently a number of dissertations,

papers, and articles have appeared that substantially challenge this interpretation. In an impressive monograph enriched by her comparative perspective, Leila J. Rupp argues convincingly for continuity over change as the key to understanding the effects of World War II on American and German women. Her two major themes revolve around popular images of women in the prewar and wartime periods and the nature and relative effectiveness of mobilization policies. Assessing the relationship between propaganda and the extent to which women contributed to the war effort, she maintains a constant comparison between the situations in Germany and the United States.

The book begins with an analysis of prewar images of women. Rupp is aware of the pitfalls in comparing Germany, which had a body of official Nazi literature, with the United States, which lacked such official ideology, and her chapter on American images, based on a sample of periodical literature, is less satisfying than the systematically documented argument that characterizes the rest of the book. While she demonstrates that in both countries the ideal was woman as wife and mother, she argues that the Nazi imperative of sacrifice for the good of the state promoted a more flexible image, one readily manipulable when women were needed in "unwomanly" roles. Thus, Nazi wartime propagandists, although altering certain emphases, did not find it necessary to transform significantly the prewar ideal of woman. In the United States, however, the more limited prewar public image changed dramatically to emphasize the woman worker. Yet the mobilization propaganda continued to stress woman's femininity, the importance of her work in bringing her man home more quickly, and the temporary duration of her new role. In both countries a national emergency necessitated alterations in the ideal of womanhood, but image makers performed those adaptations without seriously threatening traditional gender roles.

Rupp's second major concern is in comparing the nature and effectiveness of mobilization policies. The German government failed to enforce its labor conscription system and disseminated relatively little propaganda aimed at drawing women into war industry, while the United States relied exclusively on a major propaganda effort. Rejecting a simplistic interpretation of the American success and German failure in mobilizing women, Rupp moves beyond official policy and its implementation to explore other factors such as differences in financial incentives and in prewar employment conditions. Judiciously weighing these factors, Rupp is able to demonstrate that while propaganda was a significant factor in American mobilization, women were not simply manipu-

lated but acted from motivations based on their independently defined interests.

Although she makes no claim to defining the relationship between public images and the actual feelings and experiences of women—a continuing dilemma for writers of prescriptive history—Rupp presents a definitive analysis of official ideology and policy concerning women in Germany and the United States. Her extensive research, scrupulous documentation, and clear and graceful style should command the enthusiastic attention of scholars, students, and general readers.

SUSAN M. HARTMANN
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ROBERTA HAMILTON. *The Liberation of Women: A Study of Patriarchy and Capitalism.* (Controversies in Sociology, number 6.) Boston: George Allen and Unwin. 1978. Pp. 117. \$5.50.

Roberta Hamilton's work is an extraordinarily important new book for historians of seventeenth-century England and for persons trying to understand the complex elements of women's oppression. Hamilton's concern is to understand better why and how women are oppressed. Her method for tackling this question is both Marxist and feminist. The first allows her to focus on the transformation from feudalism to capitalism whereas the second helps to explain the dominant patriarchal ideology. Hamilton sees Marxist and feminist theory as complementary: "In large part the strengths of each are the weaknesses of the other" (p. 77). Marxist analysis helps explain the differences among women, feminist theory the differences between men and women.

Hamilton's argument is that the transition to capitalism redefined the position of women in the family and hence in society. Her thesis is as follows. The unity of capital and labor was at the base of the domestic economy and woman's activity. With the decline of feudalism and the rise of capitalism, the family ceased to be the economic unit of production. The decline of the family and domestic industry destroyed the interdependence between husband and wife. Hamilton discusses this change in terms of the different class forms of the feudal family—peasant, craftsman, tradesman, and noble—all of which shared the same fate as feudal society crumbled.

The major purpose of Hamilton's discussion is to distinguish clearly between preindustrial and industrial capitalism. Her position is that the fundamental redefinition of the family and hence women's lives occurred with the advent of capitalism, not industrialization. The needs of capital

required the destruction of the self-sufficient household, which happened one hundred fifty years before industrialization. Although the point of Hamilton's discussion is well taken, she overstates her case in trying to discredit functionalist analysis. She needs to keep in mind that the movement from preindustrial to industrial capitalism is a process that continued to redefine and reshape the life of the family. Capitalism may have begun the privatization process, but industrialization became a part of the process of capitalization and had a new impact on the family, the specificity of which must be understood.

Hamilton also addresses the transition from Catholicism to Protestantism as indicative of a transformation in patriarchal ideology. She believes that the challenge to the Catholic view of the "evil woman" was basic to redefining the family as a companionate though hierarchical unit. "Protestantism substituted the proper wife for the evil woman" (p. 74). Hamilton locates the reason for this switch of ideology in the early Protestant world-view itself. One still wonders what the origins of that world-view are, particularly as understood by feminist materialism. But the material base of patriarchy remains at the undeveloped stage of Hamilton's analysis. Does Hamilton mean to reduce patriarchy to the practice of ideology or does she mean to redefine the place of religion as ideology within the dialectic? Does she mean to reify the dichotomy between Marxist and feminist analysis by applying Marxist theory to the transition to capitalism and feminist theory to the study of patriarchal ideology? Or is this an unintended effect? Perhaps Hamilton will choose to write another book, equally as interesting and intriguing, in which she addresses these questions.

ZILLAH EISENSTEIN
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PHYLLIS STOCK. *Better than Rubies: A History of Women's Education*. New York: G. P. Putnam's Sons. 1978. Pp. 252. \$10.95.

This well-researched study surveys women's education from the Renaissance to the present in France, Germany, Italy, Russia, England, and the United States, and it elucidates the differences among how, what, and under what conditions men and women were taught. Women's education related to the interests of the male-controlled societies. Their formal and informal education stressed household skills and character development and neglected the intellect except in upper-class schools where certain "accomplishments" required its use.

Until the sixteenth century Aristotle's view pre-

vailed: intellectually inferior, women needed only to be trained at home in domesticity and in submissiveness to the male head of the family. Except for cloistered females, women were uneducated. Improvement came in periods of decline, such as during the breakdown of feudalism in the late Middle Ages.

From the sixteenth to the nineteenth centuries women's education was restricted when male opportunities expanded. The Renaissance was not a rebirth of learning for women, except in England where it came late during a period of the nobility's decline. The Protestant Reformation brought only modest gains. The Enlightenment set back the limited progress because of the wide acceptance of Jean Jacques Rousseau's contention that women, as different beings, should be trained at home for housewifery. Radical counterarguments had little effect.

Breakthroughs occurred in the nineteenth century when women's education was linked with that of the lower classes. Centralized states fostered primary education for both sexes to promote nationalism and/or the Protestant ethic of hard work. By 1900 primary education was becoming universal in all the states except Spain and Russia where the education of girls remained severely limited. Secondary education was still class education. The United States led in women's secondary and higher education, even though women were educated differently. Opportunities for educated women, however, were mainly extensions of nurturing roles.

All of the nations surveyed made great progress in the twentieth century. Yet, considering its early lead and its escape from wartime devastation, the United States has a dismal record in higher education for women. Statistics gathered in 1950 show losses of earlier gains, and current data are discouraging. Socially prescribed roles, low self-images, and limited employment-advancement opportunities hinder women in a nation that is not ideologically committed to making use of women's capabilities.

This is an important book with an awesome impact. The study offers evidence that traditional categories of educational progress do not fit women's education. This book should impart "wisdom better than rubies" especially to those historians who glibly dismiss vital concerns of women who desire educational opportunities and the right to choose their own social roles.

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JAMES A. INCIARDI *et al.* *Historical Approaches to Crime: Research Strategies and Issues*. (Sage Library of Social

Research, number 57.) Beverly Hills, Calif.: Sage Publications. 1977. Pp. 215. Cloth \$14.00, paper \$6.95.

This book has been designed to help forward work on the history of crime; people trained in the various aspects of criminal justice compose its intended audience, for it summarizes many scholarly areas and topics, but not criminology. The book is so poorly conceived, researched, and written that one hardly knows exactly what its aims are. The authors talk about the "problems and pitfalls [*sic*] of historical data" (p. 26), but the book itself has so many "pitfalls" that the reader comes away discouraged about the quality of any work on the history of crime. This is unfortunate, considering the substantial number of people researching the history of crime, a group with sophisticated and carefully thought out approaches to the subject. Some of the "pitfalls" of this book include a bizarre discussion of the myth of the unicorn (p. 11-16), several long sections describing what folklorists do (pp. 31-56, 148-64), and a section on Karl Jaspers and history that explores "the significance of history by examining its relationship to the concept of time and the western world's perception of reality—transcendence and telos" (pp. 119-29).

The only substantive discussion of crime in the past concerns two timeworn and under-conceptualized topics—the wild West and organized crime. The book makes two empirical contributions—one important and well put, that women were active in organized crime in the early part of the twentieth century, and the other trivial, that Joseph Valachi unreliably testified about a Mafia purge. Or, as the authors say, "Valachi was, in a sense, forced to bare [*sic*] witness to a story of which he was barely cognizant" (p. 100).

Unless one considers the grossly extended metaphors, analogies, and banalities which the authors favor as theses (e.g., on history and sex: both are "protean and problematic . . . they are among the fundamental data of human experience and existence" [p. 119]), the book has no thesis or even focus. The authors feel crime in the past should be analyzed for a "better understanding of the present" (p. 9) and that social science that does not account for historical change is naive and presentist—both sensible points but neither a thesis. And of the two subject areas explored, only one, organized crime, seems to help in achieving these goals. The authors fail to show how the anecdotes of the wild West, either in folklore or not, contribute to a deeper understanding of anything, except perhaps how folklore works, and the authors made that clear before their analysis began.

This book does a disservice to the large number

of scholars working on the social history of crime. It ignores the skill, care, and attention that most people put into their studies, substituting sloppiness and sensation. Most scholars working on the history of crime have explicit and intelligible conceptual frameworks; most see their studies as working toward a deeper understanding of society and the individual. A good many of these scholars combine relevant thinking and methods from different disciplines, but they do so in ways that benefit both the substantive historical analysis and the disciplines from which they have learned. The interdisciplinary technique suggested in this book is merely a juxtaposition: a description of issues in folklore studies adjacent to a discussion of the wild West does not constitute a methodological approach.

It appears that James Inciardi, Alan Block, and Lyle Hallowell naively conceive the writing of history to be a collecting and presenting of "true" facts: from this point of view, folklore's main weakness lies in its "*lower probability of accuracy*" (authors' emphasis, p. 50), a poor case for both folklore and history. The history of crime is not a special topic, separate from the history of society. It is important in that it helps show how society works, how it controls its members, how it handles conflict, how it defines itself, how it conceives of justice, how it deals with its most despised members, who its outcasts are, and how outcasts fit into the whole. The means to study crime are complex, ranging from legal analysis to the philosophy of justice to the study of bureaucracy and to various quantitative techniques. Although the study of crime in the past is worthwhile, it is difficult, not easy; trivia such as the age of Billy the Kid can only be useful within an intellectual framework. Otherwise, the history of crime could be relegated to the scrap heap of curiosities. One is better off reading the *Police Gazette* than this book.

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TOM BOWDEN. *The Breakdown of Public Security: The Case of Ireland, 1916-1921, and Palestine, 1936-1939*. Foreword by GABRIEL A. ALMOND. (Sage Studies in 20th Century History, number 8.) Beverly Hills, Calif.: Sage Publications. 1977. Pp. xiv, 342. Cloth \$16.50, paper \$7.95.

On title and structure alone, Tom Bowden's *The Breakdown of Public Security: The Case of Ireland, 1916-1921, and Palestine, 1936-1939* would be greeted enthusiastically by the advocates of comparative history. This enthusiasm will be even greater because his study is not potted history but a real and original contribution to the literature, based on

research in primary sources in both Israel and Ireland. In addition, he is familiar with work not simply on political violence and revolution but also on modern police forces (for most scholars still an arcane area best left to the paraprofessionals of criminal justice). Bowden has taken two unique periods of political violence—the Irish Troubles of 1916–21 and the Arab Revolt in the Palestine Mandate, 1936–39—that have certain similarities, not the least of which is the British presence, and focused on the armed challenge to public security in both areas. As he notes in his preface, “The study is then eclectic, spanning political analysis, sociology, history, military history, and policing theory” (p. xiii). It is all of that and is also good, useful, timely, and important as an approach to the past, even for those historians with little interest in the specific subject.

Still, like exercises based on historical case studies, potted or plain, the conclusions must rest on what can and should be read as conventional history. Here Bowden comes away splendidly. His three chapters on Ireland—“The Defenders,” “The Evolution of Opposition,” and “Conflict, Intelligence, and the Breakdown of Control”—are not simply a summary, but an original, analytical investigation with new material and insights even for the old Irish hand. There is a massive literature on Ireland, so to a degree this is even more impressive than the parallel three chapters on the Arab revolt, still incompletely examined and still one of the most interesting events of the long and troubled history of modern Palestine. The revolts are quite different: the elegant and sophisticated Irish revolt became the archetype of most modern struggles of national liberation while the Arab revolt was based on brigandage, tribal habits, and ancient prejudice and in Arab terms failed. The British had different problems and priorities during each revolt, and the crucial institutions of public order were quite different. Bowden, however, makes a convincing case that comparative conclusions are possible, the more so since his two major conclusions are both quite specific and yet may be useful elsewhere.

His first conclusion is that, once the system experienced severe internal stress, the crucial force for order was the police, especially police intelligence. Second, the degree of rebel politico-military development “determine[d] both the degree and the duration of the breakdown of control” (p. 308). The temptation is great to carry Bowden’s conclusions to the site of the present Irish troubles or the contemporary Arab rebels in Palestine or to expand his work on terrorism and the political police for this year’s terrorists and this year’s police. In fact, this presents the only problem with the book: despite a preface dated 1977,

the basic work was finished by 1973—there is but a single reference for 1974, and none thereafter. Thus, he notes that there are very few studies of the phenomenon of revolutionary terror, which in 1972 was true. Since then an enormous number of printed pages—good, bad, and mostly indifferent—have been churned out. Revolutionary terror is now academically trendy, a veritable scholarly cottage industry. This outpouring of literature does not mean that Bowden’s work is out-of-date in terrorist matters, rather the reverse; for the academic terrorist international could have greatly benefited if the work had come out in 1972–73, and will still benefit.

And the only other quibble is the minor and conventional point that, when historians graze in the greener fields of social science, it would be well if they left even the odd bit of argot behind. But perhaps the most impressive comment on *The Breakdown of Public Security* is that both political scientists and historians would certainly be glad to claim it as their own—an original, penetrating, timely, and, most of all, important work.

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RICHARD MILLER. *Bohemia: The Protoculture Then and Now*. Chicago: Nelson-Hall. 1977. Pp. xi, 376. \$14.95.

Bohemia is a highly selective, interdisciplinary historical sketch that pleads the case of Bohemian cultural values from the end of the Enlightenment to the era of Haight-Ashbury. Borrowing from the ideas of Erich Fromm, Abraham Maslow, and Philip Slater, Richard Miller follows Bohemianism from its modern inception in the Paris of Victor Hugo (even then identified with hashish smoking and wearing nonfashionable clothes) to the American counterculture of the 1960s, thence to a projected *Protoculture*—“a cultural mutation growing around the double helix of art and science” (p. xi).

The author’s attention is centered wholly on artistic and literary figures from Théophile Gautier to Ken Kesey. His thesis is that, as bourgeois styles became predominant in Western societies, they generated a romantic resistance to middle-class life. Miller sees Western history as a cultural war between the deadening, profit-making efficiency of the bourgeois “führers” and the reluctance of the tolerant, freedom-loving Bohemians to return to that middle class from which most of them came. What served as the Bohemian battle cry of “Liberty, Equality, Fraternity” in the time of Napoleon has now been retranslated in the late twentieth century as “Autonomy, Equality, Com-

munity" (p. 272). The author traces this proposition of Parisian Bohemias to the beginning of the twentieth century, when the French avant-garde was crushed in World War I and the German *Wandervögel* edged their way into Nazi totalitarianism.

America, meanwhile, was producing its own type of Bohemian in Greenwich Village, in New Orleans, and, especially, in San Francisco. Spurred on by black music, alcohol, and marijuana, the development of American literary resistance to middle class culture ran true from Jack London, to H. L. Mencken, to Jack Kerouac, and to Allen Ginsberg.

The hope for the future, according to the author, lies in "cultures rising not directly out of rude worker/peasant values, but from middle-class values refined from worker/peasant values from more than two centuries of living in America" (p. 286). Having neglected the political-revolutionary component of all Western oppositional subcultures, Miller can offer no analysis which would explain why the Bohemians have usually been co-opted by the middle class, or how a protoculture designed without visible means of economic support can survive.

DEAN ALBERTSON
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WILLIAM L. LANGER. *In and Out of the Ivory Tower: The Autobiography of William L. Langer*. New York: Neale Watson Academic Publications. 1977. Pp. x, 268. \$15.00.

William L. Langer intended this autobiography as an exemplary tale of how a poor boy from an immigrant family made good in America. Distressed by the iconoclasm of the sixties, he offered his life as a testament to the American dream and as a defense of the establishment—its openness to merit, its hard-working patriotism, and its contribution to American defense since World War II. It is a tribute to Langer's basic good temper and immense intellectual curiosity that the book generally escapes the narrow defensiveness of that purpose.

Langer wrote the text just before he died in 1977 at the age of 81. He recounts fondly his youth in South Boston when he, his two brothers, and their widowed German mother struggled for survival and education. Langer won entry to Boston Latin School and then scholarships to Harvard College. World War I focused his interest on modern European history, and army service provided funds to return to Harvard graduate school. Having impressed his teachers with his skill and erudition,

Langer was brought back to teach at Harvard in 1927. By 1936 he had written his masterworks on European international relations and assumed the chair established by his mentor, Archibald Cary Coolidge. Meanwhile, his marriage to the daughter of a well-to-do and cultured German-American family helped him support an amazingly rich life of music, literature, and travel.

In the mid-1920s, Coolidge had brought Langer into the Council on Foreign Relations. With his contacts there to the conservative-internationalist elite, Langer was a natural choice to become chief of the Research and Analysis Branch of the OSS. The Branch contributed significantly to the war effort and pioneered an integrated, interdisciplinary approach to policy questions, which led to the concept of national intelligence estimates and influenced development of institutes for regional studies. Langer himself set up the Office of National Estimates for the CIA and, after returning to Harvard, headed the Russian and Middle Eastern centers, before retiring from teaching in 1964.

Langer is not as successful an autobiographer as he was a historian. He indulges the nostalgia of old age too far in pleasant memories and fails to examine the sources of his historical work in depth. On issues raised by his government service, like government control and selective release of documents or the ideological bias of sponsored research and sponsored research institutes, he is impatient of criticism. On many potentially fascinating topics his comments are unremarkable. We hear little, for example, of his first wife and have to guess that she was Susanne Langer, the eminent philosopher.

Nevertheless, the inherent interest of Langer's life carries the book through. He does write about the interest in psychology which surfaced in his presidential address in 1957. As the second war with Germany approached, he became subject to anxiety attacks when he lectured before an audience and sought the psychoanalytical help of his brother's friends. They did not cure his affliction—it dogged him during his remaining productive years—but the consultations left him with great respect for analysts and the subtlety of human motivations.

It is disappointing that Langer displays little insight into his own hyphenated, German-American identity. Indeed, he seems consciously to have avoided the ambiguities of that station. A future biographer will find that silence revealing, for his mixed identity linked the traumatic event of his personal life, the death of his father at the age of two, with the traumatic public events that erupted in the First World War. Indirectly, Langer's autobiography provides clues to his patriotic identification with the establishment and to the prodigious

energy and intelligence that produced his historical works.

DOROTHY ROSS
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S. A. SKILLITER. *William Harborne and the Trade with Turkey, 1578-1582: A Documentary Study of the First Anglo-Ottoman Relations*. New York: Oxford University Press, for The British Academy, London. 1977. Pp. xxii, 291. \$38.00.

In 1580 England joined France, Venice, and Ragusa as the only European states privileged to maintain diplomatic and commercial relations with the Ottoman Empire. There is much that is unknown and perhaps forever unknowable about the background to the opening of direct Anglo-Ottoman relations. The fluidity of governance (which enabled a handful of intrepid Elizabethan merchants and a resourceful Ottoman translator to make diplomatic initiatives in the eastern Mediterranean), the sheer number of interested, indeed, jealous observers of the English achievement, and the vagaries of documentation combine to make the historian's task a formidable one. S. A. Skilliter has nonetheless managed to rescue a handsome portion of the previously unknown by piecing together some fifty Ottoman and European documents into a lucid narrative centered on the role of one William Harborne—merchant, secret agent, and ultimately first English ambassador to the Sublime Porte.

Although English ships were not strangers to Ottoman ports, prior to Harborne's mission English merchants traded in the Levant under the protection of the privileged French. France was anxious to preserve its lucrative monopoly, while Spain was no less determined to block any accord between its chief European rival and the "infidel" Turk. Elizabeth I's excommunication in 1570 and England's growing estrangement from Catholic Europe spurred interest in the Levant and the development of independent commerce. Continental religious politics dovetailed with Ottoman aspirations. Forced to replace its entire fleet after the Lepanto debacle even as it prepared to strike at Persia, the Ottoman Empire was in need of an ally, preferably one willing to trade in war materials. In defiance of the papal ban on the export of munitions to the Turks, a few English ships laden with contraband—usually in the form of bells stripped from cathedrals and monasteries—endured England to the Turks and paved the way for Harborne's diplomatic coup and the regularization of direct trade.

Notwithstanding the apparent focus on Harborne, much of the book's interest resides in the

exploratory correspondence between England and the Ottomans and the ingenuity of the Ottoman translator Mustafa Chiaus. Skilliter's study of the Ottoman and Latin texts of the correspondence reconstructs Mustafa's linguistic legerdemain, plumbs the subtleties of the diplomatic breakthrough, and points up the role of the go-between as policy maker. As the documents demonstrate, "if one can call Harborne the builder of the alliance then Mustafa is its architect" (p. 43).

Skilliter's book is both less and more than its title promises. Although Harborne is a constant figure in the narrative, he is often completely absent from the supporting documents, and the full range of his activities even during his pioneering visit to Istanbul can only be surmised. But, if the book is something less than the complete adventures of William Harborne from 1578 to 1582, it is also more than that. The diplomatic historian and even more the Ottomanist will find in Skilliter's exposition, and in his appendix of documents with their contemporaneous translations, both a careful study of the tortuous path of bilateral negotiations and an invaluable source for Ottoman diplomacies.

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S. L. TIKHIVINSKII, editor. *Tataro-mongoly v Azii i Evrope: Sbornik statei* [The Tataro-Mongols in Asia and Europe: A Collection of Articles]. 2d ed., rev. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 502. 2 r. 49 k.

This collection of twenty-two articles by Soviet and Mongol specialists, a revised and expanded version of the 1970 volume, focuses on the period and effects of the Mongol conquests in Eastern Europe, Central Asia, the Caucasus, and the Middle and Far East. In 1961, according to the editor, S. L. Tikhvinskii, the People's Republic of China began a campaign to portray Chingis Khan as a "great unifier of peoples." As a consequence, the redoubtable Mongol warrior has become a codeword in current Sino-Soviet polemics. Tikhvinskii roundly excoriates these "revisionist" circles in the PRC and indicates that the purpose of this work is to expose the "scientific untenability" of those theories that would ascribe any positive developments to the conquest period.

Despite the shrill tone adopted by some of the authors, some of the contributions have substance and are equipped with a serious scholarly apparatus. Regrettably, many are biased and few are original. The theme binding many of the pieces together is that of the negative impact of the Mon-

gol legions on this or that region. Thus, we are predictably informed that Chingis Khan played a "progressive" role in uniting the Mongols and founding the Mongol state. Once foreign conquests began, "his activity acquired a reactionary character" (Sh. Sandag). He did not truly aid the unification of peoples, rather he exterminated entire cultures, such as the Tangut (E. I. Kychanov). The real aggressor in the Mongol-Khwārazmshāh conflict was Chingis Khan (I. P. Petrushevskii). In 1230, the Mongols were considering annihilating the population of northern China and transforming the area into pastureland (L. I. Duman).

Resistance to the invaders is described according to well-known formulas. Thus, the Korean ruling class capitulated out of fear that the masses, who were arming themselves for the struggle with the Mongols, would turn on their own rulers (V. M. Serov). "Having achieved the submission of Rus', the Horde was unable to break the political structures which existed there" (L. V. Cherepnin). It was resistance in Rus' and neighboring areas that prevented the Mongols from invading Western Europe (V. T. Pashuto). Mongol and Chinese imperialism combined in the effort to conquer Southeast Asia (A. A. Bokshchanin). In contrast to current Chinese views, the revolts that marked the end of the Yüan dynasty in China had strong antiforeign feelings rather than social problems at their core in some regions (L. A. Borovkova).

S. Kuchera's "The Conquest of Tibet by the Mongols" is more original. It is marked by a careful analysis of the Chinese, Tibetan, Mongol, and other sources and concludes that Tibet did not fall to the Mongols during Chingis's lifetime. Rather, it was a more gradual process, completed by the 1250s and followed by the conquest of the Mongols by Tibetan civilization. Ch. Dalai's survey of the struggle between Qubilay and Arigh Bugha (and later Qaydu) argues that Arigh Bugha was, in fact, the lawful successor to the throne and a true Mongol patriot, who would not succumb to the blandishments of a foreign (Chinese) civilization. N. Ts. Munkuev's "Notes on the Ancient Mongols" points to some of the major problems of medieval Mongol studies—the question of the ethnogenesis of the Mongols and the continuity of politico-ideological traditions from the Hsiung-nu, Hsien-pi, Juan-juan, and Türks. He also gives an analysis of the Mongol social categories *ütögü bughol* and *nökör*. His second contribution consists of translations from Chinese sources pertaining to the wretched economic condition of most Mongol tribesmen during the Chingisid era. N. P. Shastina presents an interesting account of the positive and negative images of Chingis Khan in medieval Mongol literature. The article also contains data on the continuation of the cult of Chingis (as late

as 1956 a temple containing various relics was erected in the PRC). L. N. Gumilëv's attempt to show that the author of the "Secret History of the Mongols" was a somewhat muted anti-Chingisid member of the "Old Mongol Party" seeking a return to the pristine virtues of the Mongol soldiery is highly conjectural.

As with any large collections of essays, the quality is uneven and there are a goodly number of repetitions. Regrettably, the editor did not include a bibliography, index, list of abbreviations, or cross-references. The absence of a study on Mongol ideology is surprising. Notwithstanding the tendentiousness of some of the contributions and the obvious desire of some authors to score polemical points, this collection contains a considerable amount of useful information and cannot be ignored by those interested in the multifaceted Mongol era.

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ANCIENT

W. K. C. GUTHRIE. *A History of Greek Philosophy*. Volume 4, *Plato. The Man and His Dialogues: Earlier Period*. New York: Cambridge University Press. 1975. Pp. xviii, 603. \$37.50.

The present volume is the fourth in what is becoming a monumental and magisterial history of Greek philosophic thought. In conjunction with its predecessors, it forms an ambitious and major accomplishment of historical scholarship in our time. This book is devoted to Plato and the early "Socratic" dialogues: *Apology*, *Crito*, *Euthyphro*, *Laches*, *Lysis*, *Charmides*, *Hippias Major* and *Minor*, *Ion*, *Protagoras*, *Meno*, *Euthydemus*, *Gorgias*, *Menexenus*, *Phaedo*, *Symposium*, *Phaedrus*, and *Republic*. Presumably, the next volume will deal with the later, less dramatic, often more difficult writings. Although a case can be made for a thematic presentation of Plato's philosophy, W. K. C. Guthrie has, with good reasons, wisely decided to consider each of the dialogues as a unified expression of ideas; he discusses them in the probable order of their composition. We are thus offered concise accounts of the substance of the dialogues. But we are, in addition, provided with valuable information concerning the dramatic setting, date, historical context, persons, and allusions found in the dialogue. There are also illuminating chapters, sections of comments, and notes on points of special interest and importance to our understanding and appreciation of the dialogues.

A notable feature of Guthrie's study of Plato's

thought is his sensitivity to the "tremendous personality, one of the strangest and most individual writers who have ever lived" (p. xiii)—namely, Plato himself. At various moments in his discussions of the dialogues Guthrie prompts us, by means of apt and revealing observations, to remain aware of the somewhat mysterious yet forceful person of Plato within and behind the drama of ideas, his character, and his noticeable intentions and aspirations. There is also an excellent opening chapter on the life of Plato and the historical and philosophical influences that affected his life and thought. For the life of Plato, the one most valuable source is the Seventh Letter. But there has long been a debate among scholars over its authenticity. It is easy to pass from hoping it might be genuine to insisting that it must be. A. E. Taylor wrote, "If the *Epistles* are spurious we lose our one direct source of information for any part of Plato's biography" (*Plato* [1949], p. 14). That is true, but it does not prove them genuine. Guthrie accepts the Seventh Letter as genuine and makes effective use of it, deferring the question of the Letters to the volume on Plato's later writings.

Guthrie recognizes that there is no substitute for reading Plato himself and that a work on Plato should lead a reader back to Plato (p. xiii). But he assists his reader to this end by supplying insight, knowledge, and direction, so as to make the encounter—or re-encounter—with Plato likely to be most rewarding. With his mastery of the historical and philosophical materials, he has, nonetheless, escaped the temptation that motivated some of his great scholarly forerunners to establish a theory that explains what the philosophers had "really tried" to say—or what they "must have meant" even if they did not say so. That temptation is especially strong in writing on Plato, for the dramatic and dialogue genre invites theory and interpretation. Indeed (to indulge here in theorizing) *this might have been one of Plato's secret reasons for employing the dialogue; it encourages hypothesizing and contests over interpretation, and such contagious disputation is the stuff of philosophy.*

On all matters of surveying and weighing historical evidence and interpretations, Guthrie is scrupulous in his assessments, impartial and eminently fair in his judgments. His presentation of the historical sources and his discussions of contemporary literature and philosophizing on problematic aspects of Plato's thought is a remarkable feat of industry and critical erudition (attested to, incidentally, in a valuable bibliography, notes, and index of passages quoted).

Finally, Guthrie commands a lucid style that makes this book a pleasure to read and renders even the more recondite parts of Plato's outlook comprehensible to any earnest reader. This work

will undoubtedly become the standard history of Greek philosophy, to be consulted by specialists and laymen, historians, philosophers, and students of science, literature, the arts, political theory, and theology. Although Guthrie remarks that there can be no "final or standard work on Plato," all future scholarship on Greek thought will have a point of departure and reference here and will reflect a measure of indebtedness to this splendid history.

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WOLFGANG ORTH. *Königlicher Machtanspruch und städtische Freiheit: Untersuchungen zu den politischen Beziehungen zwischen den ersten Seleukidenherrschern (Seleukos I., Antiochos I., Antiochos II.) und den Städten des westlichen Kleinasiens.* (Münchner Beiträge zur Papyrusforschung, und Antiken Rechtsgeschichte, number 71.) Munich: C. H. Beck'sche Verlagsbuchhandlung. 1977. Pp. viii, 209. DM 56.

Wolfgang Orth's subtitle is his subject; the theme is the title. This *Habilitationsschrift* written under H. Bengtson takes up a familiar subject: the nature of the relationship of king and city. Orth's originality lies in his self-limitation to the Seleucids (and essentially the period 281–246 B.C.) and in his insistence that only a rigorous, detailed analysis of the inscriptions can lead to any real progress. Generalities are to be eschewed. There is an introduction to the problem (largely rejecting the juristic approach in A. Heuss's *Stadt und Herrscher*), a brief summary of the period before the Seleucids became a power in western Asia Minor, two chapters with the meat of the book, a brief treatment of the period 244–188 B.C., and conclusions. A map, list of cited works, and two indexes (subjects and sources) complete the book.

Chapter one is devoted to Seleucus I and Antiochus I, chapter two to Antiochus II. Each proceeds through the cities for which there is evidence. Most of this is royal letters (much of the book is a historical commentary on C. B. Welles's *Royal Correspondence*) and city decrees. Orth examines each to elicit the real underlying circumstances and relationships. The analysis is nearly always acute and intelligent; occasionally it is too clever, as Orth seeks for more meaning than is present in a text. Many details will be controversial, but there is scarcely a relevant document on which Orth does not have something interesting to say.

The conclusions are not entirely surprising: kings and cities sought their own advantage so far as their power allowed. Considerations of "inter-

national law" in a modern sense were irrelevant, and the same terminology (for example, *autonomia*, *eleutheria*) is used for widely divergent realities. None of the kings studied appears as a lover of cities, a liberal, or a defender of democracy; all sought to control the cities in their realms as far as possible. The cities uniformly sought to escape such domination, but only the most powerful (like Miletos) had any real success. The principal limits on the kings were their desire to conciliate public opinion and their need for more than military occupation of hostile territory. The Seleucids drew much of their Greek manpower, especially at high levels, from western Asia Minor. Orth has much to say about propaganda.

These views are not altogether new, but their detailed definition (the above is the baldest of summaries) and support from the evidence are a major advance, as is Orth's insistence on the variability of policies and relationships. Orth rather underestimates the cities' collective strength, and he takes the kings' weaknesses insufficiently into account; their resources of troops, energy, and cash were all very limited. Much local recalcitrance must have been ignored as not worth the bother (like tribute from some tiny states in the Athenian empire). Finally, one can accept Orth's picture and still consider the third century a period of almost unparalleled richness in the life of Greek cities. This book is a first-rate contribution to our understanding of that century.

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ROBERT J. LENARDON. *The Saga of Themistocles*. (Aspects of Greek and Roman Life.) New York: Thames and Hudson. 1978. Pp. 248. \$19.95.

The dramatic career and unusual intellectual gifts of Themistocles, creator of the Athenian navy, hero of Salamis, and, finally, renegade to Persia, have never ceased to fascinate. Herodotus attempted to delineate his character, Thucydides objectified his intelligence. Later and derivative authors, with their own rhetorical and ethical axes to grind, added to the burgeoning legend though also providing some scattered bits of factual material ignored by Herodotus and Thucydides. The ancient composite picture of Themistocles, therefore—as, for instance, in Plutarch's *Life*—is part history and part romance; disentangling these threads has been the task of modern scholarship.

The present biography, aptly entitled "saga," is devoted to no such end; instead, it presents the modern reader with most of the essentials of the ancient composite, including even a translation of Themistocles' "letters." This approach may have

its value: students, to whom this book is directed, will gain a conception of the disparate elements fused in Themistocles' "life." The author's very extensive quotation of the sources, however, is not matched by any attempt to assess their worth or to provide the reader with a yardstick by which to assess it himself. On the contrary: "Late accounts are likely to preserve authentic material and in addition actually to provide credible interpretations that cannot be ignored, even though their sources may or may not be identified. Thus any item of information that is not demonstrably false, beyond a shadow of a doubt, must receive respectful consideration; and perhaps in the last analysis all that we can do, for better or worse, is to attempt to fit together all the pieces, even those of dubious legitimacy, in the creation of a portrait, however flawed" (pp. 14–15). But students probably will not take the book *cum grano salis*; when confronted in chapter ten with the spurious letters of Themistocles, for example, they will stand helpless before them and simply read them the same way they have been reading Herodotus and Thucydides.

The book is framed as a conventional biography, with chapters ranging from pre-Themistoclean Athens to Themistocles' death, his tombs, and likenesses. Although the modern literature is cited at the appropriate points, some glaring omissions exist. Nor are the major problems thrashed out with rigor. Take, for example, the question whether Themistocles began the harbor-works of the Piraeus in 493–92, when he was archon. Thucydides' language in I. 93. 3 may imply that he commenced these works holding some continuous magistracy at a later date. Lenardon cuts the knot as follows: "Is it really too difficult to imagine a Themistocles of such persistent vision, who would doggedly and consistently pursue a naval policy from as early as 493 . . . ? I can believe in Themistocles' archonship in 493/2, with the beginning of plans for the Piraeus in that very year. The concept . . . was brilliant in its logic and practicality . . ." (p. 36).

The author's enthusiasm and his willingness to believe the utmost of Themistocles make this book engaging; it does not, however, represent an advance in knowledge.

CHARLES W. FORNARA
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ALVIN H. BERNSTEIN. *Tiberius Sempronius Gracchus: Tradition and Apostasy*. Ithaca: Cornell University Press. 1978. Pp. 272. \$15.00.

Alvin H. Bernstein has produced a valuable, well-written addition to the scholarship on Tiberius

Gracchus and his epoch-making tribunate. Bernstein's command of the ancient sources and modern research is thorough, and his avoidance of simplistic explanations for complex phenomena is admirable. For example, while properly de-emphasizing the influence of Diophanes of Mytilene and Blossius of Cumae, Bernstein correctly stresses the impact of family tradition, maternal ambition, and the Senate's rejection of the Numantine treaty upon Tiberius's actions in 133 B.C.

It is unfair, however, to present a pattern of naiveté and bad judgment in Tiberius's behavior. His return to Numantia for his financial records was not ill considered. Proceedings against Lucius Scipio Asiaticus, for example, had shown the importance of financial records in such a prosecution. Nor could he have refused the Numantines' humiliating requests at that time without jeopardizing the recently concluded treaty. Given the manpower difficulties in Spain, it also would not have been "naive or unreasonable" (p. 69) for Tiberius to have expected senatorial gratitude for saving Mancinus's army and thereby Rome's ability to mount another campaign if other Spaniards, inspired by the Numantines' military success, revolted. More importantly, Tiberius should not be singled out for adverse judgment in the deposition of a fellow tribune. That at least the powerful Appius Claudius Pulcher supported Tiberius's move is clear from his willingness to serve on the partisan agrarian commission established subsequent to this action.

The book's major weaknesses concern factional and urban matters. Although Bernstein deftly delineates the limitations of prosopography and the factional view of Roman politics, he fails to give adequate recognition to factional motives that did exist. Scipio Aemilianus, for example, in helping to keep the Numantine war alive by rejecting Tiberius's treaty not only upheld Rome's imperial interests but also won a fine opportunity for another popular military command and increased prestige in his rivalry with Appius Claudius. In return, when Claudius and his supporters backed Tiberius's land bill, they sought, among other things, to counter the popularity that Scipio would gain from conquering Numantia. The absent Scipio's associates and those who sought his favor, moreover, would not have been unmindful that they were protecting Scipio's political interests and, hence, their own by opposing Tiberius.

Although rural problems are expertly discussed, the urban aspects of the situation with which Tiberius dealt are discounted too much. Stressing the increase of slaves and freedmen at Rome, Bernstein seems to underestimate the influx of poor rural citizens and argues unconvincingly that even if there had been a sizable immigration of

such citizens, their political impact would have been nullified by reregistration in the less significant urban voting tribes. Yet illegal assumption of citizenship was difficult to prevent and ensuring proper tribal registration could not have been easy. It even may have politically benefited individual censors to leave new urban residents in their rural tribes.

Bernstein's greatest contributions lie in his analyses of the various provisions in Tiberius's proposed law and the issues it raised, the manner of its passage, and Tiberius's subsequent actions. His treatment of the historical background, constitutional context, and political implications of the decisions to appeal directly to the *councilum plebis* and to depose the obstructionist tribune, Octavius, is particularly rich and rewarding. Step by step, with welcome chronological clarity, Bernstein carefully details the transformation of Tiberius Gracchus from a traditionally ambitious aristocrat to an outright renegade whom increasingly frustrated peers finally murdered in a fit of political passion.

ALLEN M. WARD
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HELMUTH SCHNEIDER. *Die Entstehung der römischen Militärdiktatur: Krise und Niedergang einer antiken Republik*. Cologne: Kiepenheuer and Witsch. 1977. Pp. 276.

This book is devoted to the fall of the Roman Republic, from 133 B.C., the tribunate of Tiberius Gracchus and traditionally the start of the Roman revolution, through 48 B.C., when Julius Caesar became sole master of Rome and the Republic—in Helmuth Schneider's opinion—reached the point of no return. Such a study, he maintains, "enables us to analyze a social conflict in a pre-industrial society, its causes, effects, and progress over a rather lengthy period. So we can discuss the inter-relationship between economic, social and political developments, the role and importance of the pre-industrial masses in politics, and the reasons for stability and instability in the political system of a republic like Rome's" (p. 11). Schneider spends seven chapters on the subject, narrating the events and summarizing his findings along the way, and rounds out the work with a brief conclusion.

Written in very clear German, the book is intended for the informed general reader. Beginners are given a chronological table and glossary (pp. 262–72), while the more advanced can consult forty-seven lengthy footnotes recording relevant modern scholarship (pp. 245–59). But this is not the only way in which the book is simultaneously

scholarly and unscholarly. Schneider has an excellent knowledge of the political, social, and economic aspects of the first century B.C. (he covered them in his dissertation, *Wirtschaft und Politik: Untersuchungen zur Geschichte der späten römischen Republik* [1974]), and so he gives a much broader picture than those who see the republic's fall largely in terms of prosopographical or patron-client relationships. But either he knows much less about the second century B.C., or he is excessively bold in forcing its events into procrustean sociological beds, and he seriously misrepresents some bases from which the polarization of senate and people began. He has a thorough knowledge of the German and Anglo-American scholarship (he edited an anthology, *Zur Sozial- und Wirtschaftsgeschichte der späten römischen Republik* [1976]), but he ignores—or slights—the Italian and, especially, the French scholarship that might have led him to a better understanding of the senatorial mentality.

The most serious problem, however, is Schneider's preoccupation with "the masses." For years the history of the Roman Republic was written from a conservative angle; the senators were lauded as sages, the people were dismissed as a mob, their spokesmen as demagogues. Schneider ranges himself alongside the self-styled liberals who, for the moment, dominate the field. Now the Senate is a collection of selfish reactionaries, the people represent the oppressed masses struggling heroically against overwhelming odds, and the people's spokesmen are enlightened statesmen (see especially page 214 on Clodius). A detached review of the evidence suggests that neither view is adequate, since neither side possessed any monopoly on justice or morality.

But though this book lays no claim to originality, it is often more perceptive, and regularly more interesting, than the ostentatiously scholarly works on the period published of late. The propositions from which Schneider starts are suspect, but anyone working on Roman history will benefit from reading his book.

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A. V. IGNATENKO. *Armiiia v gosudarstvennom mekhanizme rabovladel'cheskogo Rima epokhi Respubliki: Istoriko-pravovoe issledovanie* [The Army in the State Machinery of Slaveholding Rome in the Age of the Republic: A Historico-Legal Investigation]. Sverdlovsk: Sredne-Uralskoe knizhnoe izdatel'stvo. 1976. Pp. 208. 80 k.

In this small book, which grew out of research for a doctorate, A. V. Ignatenko describes the inter-

actions among the complex social institutions in republican Rome that controlled and formed the Roman army. Surprisingly enough, the author departs from antique Marxist-Leninist historiography, which emphasizes the class struggle and the importance of the masses. He greatly stresses the importance of the classic Latin historians to the understanding of the time. Unfortunately, although the book offers some new insights, it has little new material drawn from Soviet archeology.

The author seems to have had little access to the works of European or American ancient historians. There are only a few references to the enormous modern literature on the Roman Republic produced outside of the Soviet sphere. Many parts of the book are extensions of the basic thesis that "in historic literature no work has examined the Roman army as suggested here. This is evident in bourgeois historiography because of its incorrect methodological approach to the study of the history of the army, which is examined without regard for the history of society. Soviet researchers, beginning from a Marxist understanding of the role of the masses, display an incomparably larger and qualitatively different interest in the sociopolitical problems of ancient history. Up to now, however, no work has appeared dealing with the social makeup of the Roman army and its political role" (pp. 8–9). Ignatenko proceeds to try and correct this situation, but the lack of evidence drawn from archeology and social history is the book's fatal flaw. Perhaps in time the author will have the opportunity to extend his research and accomplish the interesting task he has set for himself.

WILLIAM WHITE, JR.
Franklin Institute

EMILIO GABBA. *Republican Rome: The Army and the Allies*. Translated by P. J. CUFF. Berkeley and Los Angeles: University of California Press. 1977. Pp. ix, 272. \$22.00.

This excellent translation of some of the more important articles and reviews by the eminent Italian scholar, Emilio Gabba, known especially for his studies of the late Roman Republic, is most welcome. Cited frequently in the scholarly literature for this period, Gabba's name has become familiar to many, while his works themselves, in spite of their fundamental importance, have gone largely unstudied except by specialists. Now, at last, those lacking fluency in Italian can follow in detail the careful, precise, and learned arguments set forth in this collection of his writings.

The first essay concerns the origins of the profes-

sional army at Rome. Contrary to the opinion of many, Gabba convincingly demonstrates that Marius's reform of the levy in 107 B.C. was only the final stage of a century-long process. Hence, the social significance seen by many in the Marian levy seems unfounded. The second selection deals with the Roman professional army from Marius to Augustus. Here, the author argues *inter alia* that the professional army became the chief means in the first century B.C. of promotion for the rural proletariat and of advancement for the Italians in the political and social life of Rome.

There follows a somewhat more controversial essay on the origins of the Social War and Roman politics after 89 B.C. Failing to see what advantages the majority of the allies could have expected from a grant of citizenship, Gabba concludes that it was really the allied upper classes who agitated to join the citizenship body of Rome. As the fundamental significance of the Social War for later history, the author sees the development of political problems of consequence to Rome that were essentially Italian in nature rather than Roman.

Among the shorter, but nonetheless important, items that complete the book are a discussion of the problem of date and purpose of the *Lex Plotia Agraria* and two book review discussions. The latter, both of classics (*Hannibal's Legacy* by A. J. Toynbee and *Foreign Clientelae* by E. Badian), exemplify the author's sensitive and perceptive critical acumen.

We applaud the merits of this translation and express gratitude to the publisher for making these important studies of the late Roman Republic available to a wider audience.

DONALD W. WADE
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KENNETH WELLESLEY. *The Long Year, A.D. 69*. Boulder, Colo.: Westview Press. 1976. Pp. xv, 234. \$16.50.

Beginning, in standard annalistic fashion, with January 1, A.D. 69 and ending with January 1, A.D. 70, Kenneth Wellesley presents a coherent narrative of the events of A.D. 69. The content of the work, of course, derives largely from Tacitus's *Histories* (I.1 to IV.38), but Wellesley frequently changes the order of events and incorporates much material not found in Tacitus. There is little discussion of previous scholarship, although criticism of Henderson's *Civil War and Rebellion in the Roman Empire* is frequently implicit, and footnotes are limited, both in quantity and in the subjects they cover. Even so, scholars will find much of interest, for Wellesley is thorough, careful, and well in-

formed. It will not, however, be easy to use the book as a scholarly tool: on page 159, for example, it is stated that Fabius Valens was put to death at Collemancio. There is no indication that this is simply a suggestion previously proposed by Wellesley in his edition of Tacitus, *Histories* III; on the problems involved (and for details on other matters in *Histories* III), see Ronald Syme's review of Wellesley's edition in *Antichthon* (9[1975]: 63).

The most interesting additions to the Tacitean narrative are the passages in which Wellesley employs archeological, literary, and other evidence to set a scene or to establish the social and administrative context. Events in Alexandria are thus illuminated (pp. 108-14) by a skillful use of the *acta fratrum Arvalium*, the edict of Tiberius Julius Alexander, ancient accounts of the lighthouse and of the sailing time from Rome to Alexandria, the *Res Gestae*, the obelisk now in the Piazza del Popolo, Josephus's account of riots in Alexandria, and more. Similarly, Wellesley refers to aerial photography, Martial's description of a villa, the assembly of the Gauls, and many other items. The result is Tacitus broadened and made more intelligible to the modern reader.

In narrating the campaigns of the year, Wellesley does not as a rule discuss the problems involved in resolving either topographical or chronological problems. Instead, he regularly employs common sense and conjecture to reconstruct the rationale lying behind the actions of his characters, and he then gives a consistent account of the events that followed. In his account of the second battle of Cremona (pp. 141-50), for example, he provides Antonius Primus with a general strategy not found in Tacitus's narrative, and thus rather surprisingly moves further from the Tacitean version than had Henderson. Unfortunately, Wellesley's accounts of the movements of troops are frequently difficult to follow because of the one real failure of the book: the absence of adequate maps. Maps are provided only for the north Italian campaigns, and even there some of the sites mentioned in the text and crucial to an understanding of the battles are omitted from the maps (thus Sósipiro and Farisengo, p. 76).

Despite this shortcoming, this is a good book, more independent of Tacitus and more stimulating than Greenhalgh's *The Year of the Four Emperors*, and less influenced by polemic than Henderson's *Civil War and Rebellion*.

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G. W. BOWERSOCK. *Julian the Apostate*. Cambridge: Harvard University Press. 1978. Pp. xii, 135. \$12.50.

The excellent biography of Julian by Joseph Bidez (*La vie de l'empereur Julien* [1930]) made the writing of any other biography superfluous for a long time. But now new scholarship has created the need for an up-to-date biography and one, moreover, that is accessible to English-language readers. This concise yet scrupulous study admirably fits that need. It rests on a fresh and rigorous rereading of the available primary sources—literary and non-literary—not only in Greek and Latin but also, for the first time, in Syriac; on familiarity with the present state of published and much unpublished scholarship; and on a commitment to cautious and independent synthesis—it contains no bizarre theses. The sparsity of footnotes should not lead the reader to assume that this is a popular survey oblivious to technical scholarship.

G. W. Bowersock has intelligently reflected on many problems, some broad, some technical, in the study of the life and reign of Julian. He understands the interrelationship of the relevant sources. He gives the greatest emphasis in chapter two, "The Personality of the Emperor," to analysis of Julian's motives and personality, his sexual abstinence, his dissimulation, and his alienation and conversion. Some critics will question whether Julian was "a true ascetic revolutionary" (p. 20) or an ascetic conservative. Bowersock offers significant and reasonable reinterpretations of three troublesome old issues: (1) the background to Julian's acclamation at Paris in 360; (2) Julian's efforts to rebuild the temple of the Jews at Jerusalem (pp. 88–89 and apps. 1 and 2), although some aspects of that remarkable activity remain enigmatic; and (3) the establishment of the identity (probably Arab) of Julian's slayer (pp. 116–18). He prudently suspends judgment on the mysterious bull coinage (p. 104). He makes some reasonable adjustments in the established chronology of Julian's life and reign but devotes little attention to Julian's writings, philosophy, and broader intellectual activity. The book's exposition is clear, smooth, and well written; judiciously chosen quotations enliven the pages. The manuscript was carefully proofread.

The Julian who emerges from this investigation is not drastically different from Bidez's Julian; this is not a radical reinterpretation. Nevertheless, *Julian the Apostate* is an accurate and coherent distillation and refinement of present historical knowledge about Julian, and it is a handy tool for future scholarship for two reasons. First, Bowersock's familiarity with many aspects of Rome's eastern frontier and adjacent provinces and his direct consultation of the relevant Oriental sources give his biography a unique dimension. Second, his smooth integration of a vast number of conclusions from scattered scholarship, in addition to his own many critical contributions, has created a

new, more precise and subtle portrait of Julian, one that is adjusted in many factual details and nuances of interpretation. Bidez's lengthier biography remains valuable to the specialist, however, because of its footnotes and extended coverage of subjects that this work abbreviates or omits.

The appearance of this book will not eliminate the need for additional studies of Julian; many of the inadequately investigated topics listed in my review of research in *Classical World* in 1965, for example, the motivations and calculations behind Julian's decision to undertake the fateful Persian expedition, still require more attention. Highly recommended for scholars, for students, and for libraries, this work deserves an inexpensive paperback edition.

WALTER EMIL KAEGLI, JR.
University of Chicago

TOM B. JONES. *In the Twilight of Antiquity: The R. S. Hoyt Memorial Lectures* (1973). Minneapolis: University of Minnesota Press. 1978. Pp. ix, 146. \$15.00.

This book is the product of lectures given at the University of Minnesota in 1973 as a memorial to Robert Stuart Hoyt. The format employed is one of the most effective ways of presenting history, that is, history through biography.

After a brief introductory chapter surveying the political, social, and economic history of the Mediterranean area in the fourth century A.D., Tom B. Jones states that "we shall prosecute our search for humanity forsaking popes and kings, wars and pestilences, and strive to learn something about some of the representative people of the Fourth Century" (pp. 18–19). He proceeds to accomplish this in the manner familiar to readers of Eileen Power's *Mediaeval People* or E. K. Rand's *Founders of the Middle Ages*, two works, like Jones's, based on a lifetime of study and acquaintance with their respective subjects.

Jones starts the body of his work with a survey of fourth-century pilgrimages, concentrating on the Bordeaux Itinerary of 333. We also meet pilgrims such as Sister Egeria and the Holy Paula. In the next chapter we encounter the enemy, the Sassanid Persians, with emphasis on the exploits of Shapur II. This is followed by a chapter pairing the orator, Libanius, and the emperor, Julian II, the Apostate, describing the interplay between these two remarkable men, especially their confrontation at Antioch. The fifth chapter, entitled "The Churchman," deals with the Cappadocian trinity—Basil, his brother Gregory of Nyssa, and their friend Gregory Nazianzus. Through the intertwining of their lives we are introduced to the

vastly important topic of fourth-century monasticism. More obscure are the heroes of the next chapter, the unsuccessful and unhappy teacher Paladas and the soldier Abinnaeus, a "secure, stable, and well-adjusted" fortress commander in Egypt. His story is known to us from eighty-two papyri discovered in the Fayum. Next we come to the man of affairs, Synesius of Cyrene, the author of many poems and letters. Jones's description of Synesius's relationship with the famous teacher Hypatia and her tragic death is unforgettable. The final chapter, entitled "The Roman," concentrates on the life of Symmachus, the orator, with sections on St. Jerome and St. Augustine.

In concluding the book, one feels that the author has indeed accomplished his purpose—we have come to know some representative people of the fourth century and through them something of humanity. Despite these major achievements, there are some minor problems of spelling (for example, *abatoir* and *unrully*). Galla Placidia is called the wife of Alaric instead of his brother Athaulf. And pages 111–14 were missing in my review copy. Nevertheless, this will be a very useful book for general reading or for courses in the Roman Empire or the early Middle Ages.

JAMES E. SEAVER
University of Kansas

J. G. LANDELS. *Engineering in the Ancient World*. Berkeley and Los Angeles: University of California Press. 1978. Pp. 224. \$12.50.

J. G. Landels, senior lecturer at the University of Reading, is a *rara avis*, a classicist who is at home in the natural sciences and mathematics. He is, furthermore, an able craftsman: he has made his own models of some of the machines described in ancient writings and cooperated with the engineering department of his university in constructing a replica of an ancient catapult. He ranges widely for his information; he can cite inscriptions, papyrus documents, and the latest findings of archeology as well as passages from Hero and Vitruvius. On top of all this, he writes clearly and fluently. As a result, his book is a fine piece of work, a succinct and authoritative presentation of what is known of Greek and Roman engineering (the "ancient" in the title is to be taken in the narrower sense of the word).

Crisply and confidently, lightening the narrative with frequent flashes of humor and supplying easily understood diagrams where needed, he guides us through the key aspects of the subject: the available sources of energy, mostly men and animals (water-power was used to any extent only in late Roman times and then not widely; wind, save

for ships, was inexplicably never exploited; and the oft-discussed steam engines were mere gadgets); hydraulic engineering, including descriptions of some recently discovered pumps that were quite sophisticated; hoisting machinery; catapults; sea and land transport; and theoretical knowledge in the fields of hydrostatics, mechanics, and chemistry (the Greeks had a good understanding of static conditions but a poor and inaccurate one of dynamics and ballistics). A final chapter reviews rapidly the principal writings on engineering that have survived—the works of Hero, Vitruvius, Frontinus, and Pliny the Elder.

In a short book covering so much ground, some mistakes and omissions are inevitable. Landels's are all minor. In the chapter on ships he is somewhat weak on terminology: on page 136 what he calls *carvel* construction is really *shell-first* construction; on page 138 he describes *lapstrake* construction as the opposite of *shell-first*, whereas it is just another form of it; on page 138 he has confused the *hypozomata* of merchant ships, which were true undergirds passing beneath the hull, with those of warships, which girdled the hull horizontally; and on page 148 he states that naval slipways served for shipbuilding, which they almost certainly did not. In discussing the harness (pp. 173–77), he argues that the ancients used the ox-yoke for horses and mules, despite its glaring disadvantages, because horses and mules never had to pull heavy loads—in other words, why invent a better type when there was no real need? The problem, however, is not so simple: they did invent a better harness—there is evidence, dating from at least the early second century A.D., of the use of a single horse or mule in shafts (see my *Travel in the Ancient World*, plate 11)—but apparently rarely used it. An irrelevant sentence, transposed from another page, has replaced what belongs on the bottom of page 68; and on page 96 figure 32 is upside down.

LIONEL CASSON
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MEDIEVAL

R. C. VAN CAENEGEM. *Guide to the Sources of Medieval History*. With the collaboration of F. L. GANSHOF. (Europe in the Middle Ages, Selected Studies, number 2.) New York: North-Holland. 1978. Pp. xv, 428. \$40.95.

R. C. van Caenegem's *Guide to the Sources of Medieval History* is already well known from the original Dutch edition of 1962 and especially from the revision published in German two years later. This English version adds literature through 1975 and

two bibliographical chapters on historical metrology and computer-assisted research. Otherwise it is a translation, ably accomplished by van Caenegem and his wife (whose name is not given), from its German version, *Kurze Quellenkunde des west-europäischen Mittelalters* (1964).

This respected handbook sprang from instruction at the University of Ghent, where in the twentieth century Henri Pirenne, Francois Ganshof, and now van Caenegem have sustained a splendid tradition of medieval scholarship. The guide includes much material that Ganshof, who is acknowledged as a collaborator, handed over to van Caenegem. It is intended mainly for research students, though from happy experience with the first two editions, especially the German, many know that it is handy indeed for the veteran scholar.

After an introduction that acquaints one with other guides (all out of date or differently conceived from van Caenegem's), major bibliographies, and leading periodicals, the five sections of the book provide an initial orientation in (1) the types of medieval sources, (2) the libraries and archives that hold the manuscripts, (3) the development of critical scholarship and the identity of collections and lists of published sources, (4) indispensable reference works, (5) the literature of the auxiliary sciences, from diplomatics to computerized research. The bibliography is extensive and representative, but the guide is certainly not a mere bibliographical work.

Van Caenegem addresses political, legal, social, economic, and, to some extent, church historians. Those concerned with other aspects of medieval history will often find the guide helpful, but it is not primarily meant for them. It includes Byzantium, though only the Spanish part of the Islamic world.

The most valuable section of the manual, except for the pages on medieval historiography, is the typology of sources. I am surprised that in this English edition van Caenegem did not refine and simplify his classification of narrative texts in light of Herbert Grundmann's *Geschichtsschreibung im Mittelalter: Gattungen, Epochen, Eigenart* (2d ed., 1969) and Bernard Guenee's important article "Histoire, annales, chroniques: Essai sur les genres historiques au moyen âge," (*Annales: Économies, sociétés, civilisations*, 28 [1973]: 997-1016), both of which studies the author cites. Van Caenegem's types do not always draw justifiable or even clear distinctions between the various forms, and his typology rests on the questionable assumption that medieval works of history were usually "pure compilations of facts" without significant literary qualities (p. 17). In fact, these literary qualities hold the clues to genre and help one to determine how far, in what way, or whether a text is factual. An

adequate typology must grow from a comparative study of what medieval historians were expected to do as practitioners of the literary tradition.

ROGER RAY
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P. H. SAWYER and I. N. WOOD, editors. *Early Medieval Kingship*. Leeds: University of Leeds, School of History. 1977. Pp. 193. £3.50.

These six essays on early medieval kingship are based on a series of lectures given at the University of Leeds in 1977. Roger Collins's somewhat wordy "Julian of Toledo and the Royal Succession in Late 7th-Century Spain" challenges the traditional view that Julian's *Historia Wambae* is official historiography. Julian, he argues, cannot be labeled a typical episcopal or clerical writer. In "Inauguration Rituals" Janet Nelson suggests that both clerical and lay leaders saw royal anointment as socially useful. David Dumville sketches the possible motives behind genealogies and king-lists in "Kingship, Genealogies and Regnal Lists." P. H. Sawyer, in his contribution, "Kings and Merchants," stresses the role of demand in the long-distance movement of valuables and the importance of royal protection of merchants in the once-Romanized parts of Western Europe.

Of greatest interest to nonspecialists will be the fine contributions of Ian Wood ("Kings, Kingdoms and Consent") and Patrick Wormald ("*Lex Scripta* and *Verbum Regis*: Legislation and Germanic Kingship . . ."). On the basis of scanty evidence Wood plausibly argues that the territorial division at the death of Clovis in 511 was the result of political compromise and not the carrying out of a fixed tradition. Subsequent divisions of the Merovingian kingdom were likewise the outcome of immediate events. In his provocative analysis of barbarian law codes Wormald doubts that they were of much use to sitting judges. Indeed, the codes were intended to project an image, not to codify or revise existing law. The barbarian kings used the codes to portray themselves as latter-day Romano-Christian rulers. The monarch's *verbum* gave the laws their legal force. Wormald's sensible approach helps to account for the many peculiarities in barbarian legislation. I would hope that Wormald would now attempt to identify the specific characteristics of the king-images in the various codes. Finally, we need to know more about the audience to which the codes were directed. I doubt that the kings were trying primarily "to impress themselves" (p. 136).

All six authors are exceedingly reluctant—perhaps overly so—to draw general conclusions about kingship. Rather than speculate on theories or

intellectual traditions, they prefer to focus on the accidents of *de facto* political forces. They imply (p. 3) that Carolingian kingship was partially a continuation of the loose, nontheoretical legacies of Merovingian practice.

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JACQUES HEERS. *Parties and Political Life in the Medieval West*. Translated by DAVID NICHOLAS. (Europe in the Middle Ages, Selected Studies, number 7.) New York: North-Holland. 1977. Pp. viii, 312. \$37.00.

Devoted mostly to Italian history, written by a Frenchman for a series edited in England, translated (competently) by an American scholar, issued under the imprint of a Dutch publisher, and printed (poorly) in Singapore, the publication of this book represents a minor triumph of international cooperation. Even so, its author, Jacques Heers, known best for his past contributions to Genoese history, does not adopt a particularly conciliatory tone toward those scholars who through the years have written on medieval politics. Their interpretations are dismissed as "erroneous," "simplistic," "puerile," "useless" (p. 16), and generally "worthless" (p. 222). Particularly harsh judgments are meted out to scholars suspected by Heers of adhering to a materialist interpretation. To some degree such polemically blunt language may be justified. A nineteenth-century parliamentary and constitutional model has shaped many interpretations of the politics of Florence, Venice, and other Italian and non-Italian medieval and early modern governments. And sometimes this model has been discarded in favor of a no more satisfactory vulgar Marxism that it is quite appropriate to dismiss.

Heers seeks to remove his discussion from liberal-conservative, noble-bourgeois, bourgeois-proletarian antinomies. Instead, he applies to the study of predominantly urban politics from the eleventh to the sixteenth centuries categories generally used by anthropologists in their studies of premodern societies—kinship, clientage, and ritual. He concludes that late medieval politics was dominated by aristocratic families, neither exclusively urban nor rural in character, whose members were directly responsible for the agitation and turbulence that punctuated the political history of European cities during those centuries. "Parties" were formed around these families, drawing their leadership from the clans and neighborhoods to which the families belonged; members of those parties were drawn from all social ranks. Each party possessed its symbols, competed for the

emoluments of power with other local parties, and sought, above all, to preserve the honor of its members while banishing competitors. Party conflict was not ideological, and it was certainly not rooted in economic or social differences. Rather, the "competitive spirit" generated the political conflict of the age. Based on extensive, if occasionally eclectic, reading of many published sources dealing with Italian and non-Italian history, this book offers a wealth of examples that in Heers's estimate substantiate his generalizations.

Despite the interesting ideas contained in this book, Heers's narrative is often marked by a mechanical quality reflected both in the book's style and in its conclusions. Stylistically, the book is marred by the author's seeming propensity to string out the contents of note cards compiled while he read his numerous sources. Often, too many examples are piled up in support of a point, endowing the book with a manualistic air that not even the author's polemical stance helps to dissipate. Substantively, it is clear that Heers has taken much too negative a position against those authors he labels as economic determinists. Even non-Marxist historians, such as Niccolò Rodolico or Lauro Martines, whose conclusions do not accord with those of Heers, are not referred to by the author. The issues are presented throughout in an overly simplistic and therefore distorted manner. One example must suffice in this context. Without offering a specific page reference, Heers claims that in his well-known article ("The Ciompi Revolution," in N. Rubinstein, ed., *Florentine Studies* [1968]) Gene Brucker concluded that "the great magnates . . . were largely responsible for the Ciompi uprising of 1378" (p. 126). At the very least, this is a caricature of Brucker's considerably more subtle position, which, it is interesting to note, Brucker softened even more in recent work. In numerous other examples, Heers proceeds to establish precisely the kind of strawmen that a scholar of his erudition has little difficulty in demolishing. Although he rejects the positions of vulgar Marxists, it appears to me that a kind of vulgar positivism—whose ingredients are the elite theory of Mosca, Michels, and Pareto; excessive faith in prosopography; and a commitment to turn-of-the-century French views of crowd psychology—has insinuated itself into Heers's interesting if rather partisan and factious interpretation of political life in the medieval West.

ANTHONY MOLHO
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PATRICK J. GEARY. *Furta Sacra: Thefts of Relics in the Central Middle Ages*. Princeton: Princeton University Press. 1978. Pp. xiv, 227. \$14.50.

The cult of relics in the Middle Ages is coming into its own as a subject of serious historical investigation. Scholars on both sides of the Atlantic are examining its many facets and this monograph, handling a topic largely ignored in the past, represents the most significant American contribution to date.

After establishing the importance of saints and their relics in the "central Middle Ages" (ca. 800-ca. 1100), Patrick J. Geary opens his investigation of thefts *per se* by describing the activities of the professional relic thieves of the ninth and tenth centuries. He then examines a series of theft accounts from Aquitaine, northwestern France, Germany, and Italy, most of which date from the eleventh century. Geary argues that during this period saints and their relics provided communities, whether religious or urban, with a variety of material as well as spiritual benefits and that when a community felt threatened it often sought to acquire a new saint as a form of "crisis intervention." In such a situation a theft, or the assertion of a theft even when none had actually occurred, offered certain advantages over other means of obtaining relics. For example, the story of a theft emphasized the value of the object in question and allowed a "new owner" to assert possession of a relic already claimed by another locality. Despite their popularity, thefts never became fully acceptable, and Geary investigates how the thieves' literary attempts to justify their actions developed into "a particular hagiographic tradition, that of *furta sacra*, which had its own limitations, *topoi*, and forms" (p. xi).

Geary is at his best in unraveling the tangled accounts of individual thefts to suggest the reasons for their occurrence and in describing the central role of saints and their relics in this age. His exposition of the medieval view that saints resided with and participated actively in the affairs of the communities possessing their relics is essential to understanding the function of saints in this society and the desire of communities to steal or, as he argues, to "kidnap" them. His discussion of the development of the literary tradition is less satisfactory. The stress placed on this topic in the introduction and conclusion leads the reader to expect a more sustained treatment of it than the book provides. Points essential to this theme are diffused throughout the work and never effectively united. Furthermore, while the author rightly emphasizes the cultural context of the theft narratives, their literary context—their relationship to translation accounts in general—deserves more detailed consideration, for many of the rationalizations for the furtive transfer of bodies were used to justify solemn, public translations as well.

The author compensates for occasional typo-

graphical and factual errors, such as the confusion of Marcellinus the priest with the homonymous pope (p. 137), by citing the BHL numbers of hagiographic texts. These references and especially the handlist of relic thefts appended to the work will provide valuable aids to the scholars who will surely pursue numerous aspects of this seminal study.

JOHN M. MCCULLOH
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ALFRED P. SMYTH. *Scandinavian Kings in the British Isles, 850-880*. (Oxford Historical Monographs.) New York: Oxford University Press. 1977. Pp. x, 307. \$24.00.

This study is an attempt to flesh out the careers of several Scandinavian leaders presumed to have settled in Britain in the third quarter of the ninth century. The result is more romance than history. Central to the narrative are Ragnaar Lodbrók and his sons Ívarr, Hálfðan, and Ubbe, just slightly reduced in stature from their role in thirteenth-century Icelandic heroic saga. Although now shorn of any supernatural endowments, members of this family still manage in Alfred P. Smyth's account to rule the Hebrides (849) and Dublin (851), engage in the Mediterranean slave trade (858), and lead the great army of Danes against eastern England (865). As devotees of Odin, they are inclined to sacrifice prominent ecclesiastics and to carve blood-eagles on the backs of kings. Complicated genealogies, source comparisons, and maps of military movements lend an air of authenticity to the whole.

A main theme of the book is the intimate connection between Scandinavian attempts at conquest in England and in Ireland. Smyth's chief card here is his identification of Inwær (named in the *Anglo-Saxon Chronicle* as a Danish chieftain) with Imhar (mentioned in Irish sources as one of the leaders of Norwegian Dublin); both names are the same as Old Norse Ívarr, son of Ragnaar Lodbrók. (The identification is not new; it was last rejected by Nora K. Chadwick [1959] and persuasively defended by Francis J. Byrne [1963]. Smyth cites neither.) Other connections made by the author between individuals bearing similar names seem more haphazard: for example, Óláfr inn Hvíti of Dublin is identified with Óláfr Geirstadaálfr, king of Vestfold, who is said to be the arthritic skeleton in the Gokstad ship burial; Caittil Find, named in the *Annals of Ulster* as the Norse leader defeated in 857 by Óláfr and Ívarr, is equated (not for the first time) with Ketill Flatnef, a famous Viking of thirteenth-century saga. These identifications—like the author's romantic narra-

tive—are achieved by combining sources of widely differing character, age, and historical value. The result is a coherent story that entertains (Smyth's own description of the function of Norse saga), but it is not history so much as a house of cards that collapses if anyone is churlish enough to sneeze.

Smyth spares his readers facts that seem to confuse or contradict his narrative. In order to link Ragnarr Lodbrók with the Regenheri who sacked Paris in 845, he withholds the contemporary statement of the *Annales Xantenses* that the Viking leader died soon after the raid. Smyth never confronts the rather basic issue of whether Ragnarr Lodbrók ever existed, historically, as one and the same individual. The two names are nowhere coupled in the relevant Continental, English, or Irish sources for the Viking period; indeed, the name Lodbrók does not occur before 1070 and is not linked with a Ragnarr until the twelfth century. Nor is there certain evidence that Hálfðan, Ívarr, and Ubbe were brothers, let alone sons of a man whose name corresponds to Ragnarr (see R. W. McTurk, "Ragnarr Lodbrók in the Irish Annals?" *Proceedings of the Seventh Viking Congress* [1973], pp. 93–123). Smyth does not attempt to distinguish chronologically the various layers of source material at his disposal; nor can he explain the rapid proliferation and possibly topical appeal in twelfth- and thirteenth-century Europe of a legend recounting how in the ninth century the sons of Ragnarr Lodbrók conquered England.

These are serious flaws. *Caveat lector*: many of the certainties upheld by the author are uncertainties; and his proclamation that "there can now be little doubt . . ." too often signals that the time for disbelief has come.

ROBERTA FRANK
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H. R. LOYN. *The Vikings in Britain*. New York: St. Martin's Press. 1977. Pp. 176. \$14.95.

Few episodes in European history are as dramatic as the raids and settlements of the Vikings, whose movements were the last of the migration age in the West. Their impact on the Franks and Saxons came at a time when those peoples had learned to think of themselves as the heirs of the Roman Empire, without altogether abandoning the practices of their own history. Consequently, we know much about some aspects of the Vikings because literate churchmen wrote of their behavior with abhorrence but surprisingly little about other aspects because their behavior allowed them to merge into societies that were not so very different from their own.

For British historians, this contrast leads to a

perennial fascination with "The Effects of the Scandinavian Invasions on Britain"—the title of the final chapter in H. R. Loyn's book. There has been such a trend away from the Wessex-orientated view, propagated by most of the documentary sources, that the Vikings have recently been in danger of being seen as "long-haired tourists who occasionally roughed up the natives" (a telling criticism by J. M. Wallace-Hadrill). Loyn approaches the subject with breadth of knowledge and critical judgment that will make his book a most welcome addition to the literature. It is a synthesis that draws on all forms of evidence, usually giving each the weight that it deserves. The author carefully demonstrates to the reader that which is generally agreed upon, that which is in the balance, and that which is plausible but far from proven. The book has the further merit of a geographical range, discussing in appropriate depth different evidence and problems from the various regions of the British Isles.

Specialists may make particular criticisms; as an archeologist, I am not sure that Loyn should use the slighting of the defenses at Cadbury and Cricklade to illustrate Cnut's confidence (p. 95). The settlements are very different in type, and it is not certain that Cricklade's wall was pulled down in Cnut's reign. All that, however, is petty against the far more important lessons that Loyn demonstrates in his consideration of Cnut and his "empire," held together by the wealth of England.

The seven chapters in the book follow an orthodox pattern: a review of the state of affairs in Scandinavia, Europe, and Britain before the raids; the narrative sequence; and the state of play as the Viking era ended and Europe no longer felt the impact of Scandinavia except as a relatively minor trading partner. There are brief footnotes that point students toward the most important literature on the various topics and, finally, a sensible bibliography. The book is well produced, although more text drawings would have been helpful. There are a few minor misprints, including a rather alluring "Jar shof" for "Jarlshof" in the list of illustrations.

Loyn's *Anglo-Saxon England and the Norman Conquest* has long been the best basic book for students of the period, for it is well written, authoritative, and comprehensive. *The Vikings in Britain* is now its boon companion.

DAVID A. HINTON
University of Southampton

ROY MARTIN HAINES. *The Church and Politics in Fourteenth-Century England: The Career of Adam Orleton, c. 1275–1345*. (Cambridge Studies in Medieval Life and Thought, Third Series, number 10.) New

York: Cambridge University Press. 1978. Pp. xiv, 303. \$29.50.

Edward II (1307–27), vindictive, changeable, and incompetent, was probably the worst king in English history. Many of his associates appear equally unwholesome. Edward's disasters were largely of his own making, and he faced them with a singular lack of grace. He had a notorious weakness for venal male favorites, especially Piers Gaveston and Roger Despenser the Younger, and his defeat, deposition, and death were largely brought about by his own wife, Queen Isabella, and her paramour, Roger Mortimer. Not surprisingly, it has been generally thought that the bishops of his time, even if they avoided his sexual eccentricities, probably mirrored his other failings. Roy Martin Haines of Dalhousie University now seeks to revise that general picture and, in particular, to rehabilitate the career of Adam Orleton, one of the mightiest Edwardian prelates and the bishop usually considered most directly involved in the king's demise.

Haines has set himself a formidable task, for the weight of medieval testimony and recent scholarship is clearly against him. The magisterial authority of William Stubbs, T. F. Tout, and Kathleen Edwards supports the contemporary opinion that Bishop Adam was a scandalous self-seeker, a traitor to his king. Thus, courage and skill, which are always needed to attempt the biography of any medieval figure, are here doubly required to interpret the intractable evidence that so rarely indicates personal motives. Haines again demonstrates the merit of the struggle; his carefully detailed, closely reasoned analysis exposes the false prejudice of the principal chronicle witness and reveals the improbable, unwarranted nature of much of his account.

Orleton's career was determined more by Rome, or by Avignon to be precise, than by Westminster. On his own initiative Pope John XXII made Orleton bishop of Hereford in 1317. Edward II thundered against this supposed invasion of his regalian rights but eventually became reconciled to the choice and continued to use Adam as an envoy to the pope and the French king. Such missions frequently kept the new bishop abroad and perhaps partly account for the rough times he experienced upon returning to his diocese.

By 1321 Orleton had become caught up in the widespread opposition to King Edward, and he eventually urged the deposition of the monarch and the coronation of his young son, Edward III. The extent to which Adam led this movement or merely reacted to forces beyond himself remains unsettled. He does not appear to have been a

partison of Queen Isabella before 1326 and must have been shocked at the way she and Mortimer controlled affairs after the king's murder. At any rate, even before Edward's death, Orleton returned to the safer world of diplomacy. With dozens of retainers he paraded through Europe's capitals and was even advanced by John XXII to the bishopric of Worcester in 1327 and to that of Winchester in 1333. His missions failed to prevent the outbreak of the Hundred Years War, however, and as a saddened man of poor health and minimal eyesight he remained quietly in his last diocese from 1338 until his death seven years later.

One of the many neglected facets of fourteenth-century life that this fine study illuminates is the extent to which the depravity of the court was reflected in the disturbances of the countryside. Orleton was a painstaking, if largely absentee, bishop. He promoted monastic reform, ordained hundreds of clerics at a time, and even confirmed little children brought out to him as he passed along the highway. But he supported few young clerks for foreign study, was frequently in debt for cathedral repairs, and was often beset by rebellious parishioners and officials who were not above interrupting his ceremonials, robbing his baggage, and roughing him up. Most contemporaries clearly considered him inordinately ambitious. Haines's defense of Orleton is not too persuasive at this point, but he does exonerate the bishop of the cruel, vengeful streak others have attributed to him.

Orleton was a diplomat, bishop, and politician, but by organizing his examination under these headings rather than following a more chronological pattern Haines somewhat obscures the shifts in the bishop's policy and intentions. On the other hand, he does emphasize how difficult the times were and especially how divided men's loyalties had become. Prelates like Orleton probably tended to segment their own lives. Certainly the good that Haines finds in his career comes from his routine performance of ecclesiastical duties, not from any outstanding service for his country or from any uniquely generous aspect of his character. Even this improved picture of a fourteenth-century bishop indicates that, just beneath its pageantry, England had many depressing characteristics in the days immediately before the Black Death.

EDWARD J. KEALEY
College of the Holy Cross

JENNIFER M. BROWN, editor. *Scottish Society in the Fifteenth Century*. New York: St. Martin's Press. 1978. Pp. xi, 273. \$19.95.

Jennifer M. Brown rightly points out that there has been comparatively little written about fifteenth-century Scotland and that what has been written deals mainly with the political conflicts of the period. Giving credit to R. G. Nicholson for his substantial volume on medieval Scotland in the *Edinburgh History of Scotland*, the editor justifies this collection of specialized articles by demonstrating the need for "a more balanced analysis of the period by bringing together surveys of a wide range of topics by specialized scholars" (p. 7). Two who are not historians, John MacQueen, a well-known expert on Scottish literature and director of the School of Scottish Studies at the University of Edinburgh, and James J. Robertson, a senior lecturer in law at the University of Dundee, add an interdisciplinary perspective and balance to the book.

The first article, by Norman A. T. Macdougall, examines the sources surrounding the legends attached to James II and James III and clearly blames sixteenth-century historiography for "misconceptions and embroidered fantasies" regarding James III (p. 12). Macdougall is unequalled because of his voluminous knowledge of James III, but this knowledge really requires a book in itself. Brown's article, "The exercise of power," examines the relationship between the king and the lords and argues that there was as much cooperation as conflict in the fifteenth-century Scottish political scene. S. G. E. Lythe, who is quick to point out the limitations of fragmentary evidence, provides an interesting evaluation of economic life in Scotland during this period and concludes that the Scottish rulers had very little influence on the economic life of their country. Nevertheless, land tenure and the status of the mercantile class were changing and could be enhanced by parliament or king. Barbara E. Crawford and Macdougall team up in chapter five, "Scotland's foreign relations," to show that Scotland had an important role in determining foreign policy in Scandinavia, England, and France. In "Church and society" Ian B. Cowan gives an excellent survey of the interrelationships between the Scottish church and the people it served and influenced. An outstanding example of its role is the founding of the three great universities—St. Andrews, Glasgow, and Aberdeen—by churchmen in their respective cathedral cities in the fifteenth century. In a century that until recently appeared lawless and violent, Robertson shows the importance of parliament, professional lawyers, and ecclesiastical law and courts on the development of law in Scotland. Geoffrey Stell, with numerous plates and diagrams, distills out of standard works and original sources the changing architectural developments in the late fourteenth and fifteenth centuries. "The

literature of fifteenth-century Scotland," with its innovative writers, is given a scholarly analysis by MacQueen. In the final article, John W. M. Bannerman, lecturer in Scottish history at the University of Edinburgh, gives a thorough examination (127 footnotes) of the Lordship of the Isles. He sees the lordship as a major contributing factor in the preservation of Gaelic language and culture in Scotland.

This collection of articles provides scholar and student with an in-depth knowledge of important areas of Scottish studies in the fifteenth century; but the lack of continuity between the articles and their placement hinders the book's readability. Two of the authors make reference to Reg. of Supp. 391, f. 223v, and there seems to be some confusion about the participants in a murder. Also the foundation of King's College, Aberdeen, seems to be in doubt. Like any specialized articles, these raise as many questions as they answer, especially the articles written by Robertson, Lythe, and Bannerman. A valuable chronological table as well as a very limited bibliography are additional aids to readers. Obviously, more work needs to be done on fifteenth-century Scotland, as well as on other periods in its history; this book will give direction to graduate students on both sides of the Atlantic.

CHARLES H. HAWS

Old Dominion University

JOHN BOSWELL. *The Royal Treasure: Muslim Communities under the Crown of Aragon in the Fourteenth Century*. New Haven: Yale University Press. 1977. Pp. xiv, 526. \$25.00.

This richly textured account of the social lives of the Muslim minority in the fourteenth-century Crown of Aragon is a significant contribution for a number of reasons. First, it compares throughout the situation of Aragonese *mudéjares* with the very different status of their brethren in Valencia. As John Boswell makes clear in a forceful concluding statement, the social status of Muslims was determined by structural features of intergroup relations and not by the putative tolerance or intolerance of individual Christians. Therefore, Aragonese Muslims enjoyed the rewards of a more pliable and comfortable *modus vivendi* than did the Valencians, vastly greater in number and less acculturated.

Second, Boswell has chosen to present Muslim-Christian interactions in an epoch of stress, the decade when Aragon was at war with Castile (1355–66). This research strategy was very imaginative because, as one might expect, crisis tends to sharpen lines of social cleavage and also to elicit self-conscious statements of social values that in

less troubled times may go unarticulated. Thus, as the fiscal demands of the crown increased, bringing excruciating pressure on the already depleted reserves of the Muslim communities, these were nevertheless able to capitalize on their indispensability to the crown by formulating and winning redress of grievances, stated in detailed lists that Boswell includes as a check upon his own perception of those grievances.

These two facets of exposition tie together neatly. In noting the mass defections to Castile by Valencian *mudéjares*, Boswell concludes that loyalty to the crown was a function of degree of acculturation. Aragonese Muslims, most of whom were monolingual Romance-speakers and the legatees of several hundred years of culture contact, had more to lose than did the Arabic-speaking Valencians. This line of explanation is sufficient to the point, and Boswell's rather florid characterization of collective disloyalty as a tradition in Islamic Spain does not illuminate it further.

Most of the narrative is devoted to thematic discussions of the internal structure of Muslim communities, of the fate of Muslims before a dual legal system (Islamic and Aragonese), of duties and taxes owed by Muslims, and of Muslim legal rights. The picture that emerges is one of a subjugated group, but one not much worse off than the Christian peasantry, with still some semblance of the autonomy promised them in the treaties of capitulation. There follows, however, an absolutely chilling discussion of oppression in which Boswell shows how the heightened powerlessness of the religious minorities in medieval Spain rendered them susceptible to special horrors—in particular, the sexual exploitation of Muslim women.

In the final analysis, Boswell demonstrates conclusively that the Muslims were caught in a trap that made their oppression inevitable: branded as an inferior religious group but still an important source of revenue for the dominant society, the *mudéjares* had to endure the persistent insecurity that followed from the Christians' own ambivalence.

THOMAS F. GLICK
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MONIKA ZMYSLONY. *Die Bruderschaften in Lübeck bis zur Reformation*. (Beiträge zur Sozial- und Wirtschaftsgeschichte, number 6.) Kiel: Walter G. Muhlau Verlag. 1977. Pp. 256. DM 25.

Late medieval confraternities were social, charitable, and religious institutions. Monika Zmyslony's thoroughly researched thesis describes Lübeck's confraternities using her own research (hampered by difficult access to parts of Lübeck's

Stadtarchiv in the German Democratic Republic) and the rich historiographical tradition of the city and its confraternities. Apart from a weak first chapter that tries to sketch in twenty pages the German corporative tradition and the founding of the mendicant orders, the book provides a good summary of local conditions. Chapter two lists the sixty-seven known confraternities, discusses the saints to whom they were dedicated, and gives general features of their activity. The heart of the book is chapter three, in which the author studies the confraternities of different social groups. The oldest confraternities, those of the clergy, all included some lay people; the largest and richest had the largest lay element.

Five confraternities to which the city's merchants belonged were the most important. Their overlapping memberships ranged from twenty-five to forty men and their wives. Membership and leadership generally corresponded to that of the ruling elite. Clerics, excluded from holding office, played a small role. Yet religious confraternities, such as those of vicars, seamen, city employees, and musicians, were clearly also occupational groupings. Journeymen did not belong to the confraternities of their masters, however, and the town council did not permit butchers to form confraternities because of their role in the riots of 1380–84. One merchant confraternity included those (usually of south German origin) who were involved in Lübeck's considerable trade with Nuremberg in the late fifteenth century; the confraternity also served as immigrant association and chamber of commerce.

In chapters four through seven Zmyslony considers other aspects of confraternities—festivities, particularly the often sumptuous annual dinner; social composition and charitable activities, such as the individual prebends for certain of the city's poor; organization, in which membership varied from twelve to one hundred and fifty, governed by two to eight elders; investment, sometimes handled by front men (*Treuhändler*), where the law forbade well-endowed confraternities, like churches, to own land or rent out property in the city; and, finally, the continued existence of confraternities after the Reformation, with some surviving as charitable institutions under city supervision until 1846.

Zmyslony's study unfortunately refers too little to the history of Lübeck, its church and society. Instead of discussing the local context, the author usually introduces a topic in the broadest terms possible and then describes its local manifestation. Thus we have four pages on the cult of St. Gertrude in general (pp. 116–21) and one page on the corresponding confraternity in Lübeck. The general observations and extensive bibliography omit

several important studies (articles by LeBras, Duhr, Meersseman, and Hoberg, for example). The book is nevertheless useful as a summary and amplification of previous research and a resource for those who would write the religious history of medieval Lübeck or study the institutional aspect of confraternities.

CHARLES MCCURRY
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KAROL GÓRSKI. *Żakon Krzyżacki a powstanie państwa pruskiego* [The Teutonic Order and the Origin of the Prussian State]. Wrocław: Ossolineum. 1977. Pp. 245. 85 Zł.

The doyen of Polish scholars working on the Teutonic Knights and their regime in Prussia has revised his 1971 study, *L'ordine Teutonico: Alle origini dello stato Prussiano*, for publication in Poland. Karol Górski narrates the political history of the order from its foundation in 1198 and establishment in Prussia after 1230 through the secularization of the Prussian state by Albrecht von Hohenzollern in 1525. He intersperses in this analysis of society and administration in the order's state brief passages of historiographic comment or polemic. But since Górski has provided no scholarly apparatus and only a select bibliography of secondary studies, and since he rarely engages the sources directly, his book is for a general audience and is not the full study that specialists would welcome.

Górski avoids the chauvinism that has so often marred this subject, long an object of acrimony between German and Polish writers. His historiographic remarks are balanced, and his treatment of Germanization in fourteenth-century Prussia is a sensible discussion of sociocultural change. Górski achieves his greatest success in the second half of the book, where he draws extensively on his own earlier research to moderate extravagant claims for the modernity and efficiency of the order's state in Prussia and where he skillfully places the struggle between the Prussian estates and the order in the widest context of late medieval corporatism.

Only two aspects of *Żakon Krzyżacki* detract from its suitability for the intended audience. The author's modern Polish perspective is, of course, unavoidable and, in a field quantitatively dominated by German scholarship, a point in its favor. But a study of the Teutonic Order ought to reconstruct the order's perspective and portray the world as seen from Marienburg or Königsberg, not, as occurs too often here, from Cracow. The discussion of the early order and its conquest of Prussia contains less substantive detail and histori-

cal context than do the best portions of the book. This simplification can mislead an uninformed reader. When Górski imputes to Grand Master Hermann von Salza well-formed plans for a new state and when he implies a necessary opposition between religious and political considerations in the thirteenth-century order, he strains the scanty evidence and simplifies a complex reality. To see the Teutonic Knights as monks engaged in what should have been a task of converting the heathen and to criticize their failure to fill this role is to neglect the previous history of the Baltic mission (here discussed briefly only after an assessment of the Prussian conquest) and the relevant experience of other military orders in crusader Palestine. What exactly the Teutonic Knights thought they were about in thirteenth-century Prussia and how they went about it remain, as they do in the older scholarship upon which Górski draws, unresolved issues.

Such caveats aside, here is a useful survey by a knowledgeable and careful scholar, a book that perhaps deserves the translation that would make it accessible to those who could best profit from it, medievalists and others who lack Polish. The specialist who can now read it will continue to rely more on Górski's earlier and more scholarly works.

RICHARD C. HOFFMANN
York University

DAVID MARSHALL LANG. *The Bulgarians: From Pagan Times to the Ottoman Conquest*. (Ancient Peoples and Places, number 84.) Boulder, Colo.: Westview Press. 1976. Pp. 208. \$18.75.

The "Ancient Peoples and Places" monograph series is devoted to studies of individual ancient and medieval societies. These studies are meant to be at the same time scholarly in conception and presentation and geared to the "nonspecialist." To this latter end the volumes are short and graced with a large number of illustrations and photographic plates. David Marshall Lang's volume on medieval Bulgaria, too, is short and has good text figures (mostly after Naslednikova), ground plans and maps, and sixty-two photographs accompanied by useful notes. It treats the geographic environment in which Bulgaria developed, the ancient ethnic and historical background of the area, and the migrations that contributed to the formation of the Bulgarian people, and includes, of course, a narrative survey of the First and Second Bulgarian Empires. There are also chapters on social protest (the Bogomil movement), literature and learning, and architecture and the arts, as well as a good bibliography (largely in Bulgarian).

This book, however, is not among the more successful in the series. Lang's outline of Bulgarian history, which emphasizes the Turkic element in early Bulgaria, is too dry and detailed for a work of this sort and, like the topical chapters, is occasionally marred by mistakes and inaccuracies. Generalizations, sometimes unexpected ones, are stated rather than evolved: Hesychasm, according to the author, was "a thoroughly decadent and degenerate movement" (p. 117). Indeed, interpretive sections sometimes recall the chauvinist textbooks of the interwar period of twentieth-century Balkan history—Bulgarians are "dogged fighters in what they regard as a just cause" (p. 20). Further, some of the author's judgments seem to be contradicted even by the evidence he presents; thus, a leading exponent of the "degenerate movement" called Hesychasm was responsible for a school, the renown of which spread throughout the Orthodox Balkans (p. 117).

For their introduction to medieval Bulgaria, general readers or interested students should turn to Robert Browning's recent *Byzantium and Bulgaria* (1975) or to the classic treatment of Steven Runciman, *A History of the First Bulgarian Empire* (1930).

GEORGE P. MAJESKA
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MODERN EUROPE

HARRY A. MISKIMIN. *The Economy of Later Renaissance Europe, 1460-1600*. New York: Cambridge University Press. 1977. Pp. x, 222. Cloth \$15.95, paper \$4.95.

A study of the economy of early modern Europe that begins with witchcraft and ends on the death bed of Grotius must be considered unusual by any count. In this case, it carries some weight, for within the setting Harry A. Miskimin has produced a subtle and intelligent survey of the late fifteenth and sixteenth centuries, first the recovery from the ravages of plague and then the material expansion of the communities of the Continent. The keynote of the first chapter is an elaborate review of the Scholastics and of attitudes to property, taking up some of the themes discussed by Douglass North and R. P. Thomas. In the absence of a convincing synthesis for the fifteenth century, the task is far from easy, but the important questions are there: what were the constraints on the power to rule, the competence of institutions, and the effectiveness of controls? The Venetian ambassador could note that Francis I ruled his subjects harshly, but how harshly must still remain a question in the margin of our knowledge.

The argument then turns to the recovery after

1460, when crises ebbed and gave way to expansion. If the total size of Europe's population still remains in some doubt, it is abundantly clear that most cities prospered during the sixteenth century: Venice and Antwerp, Seville and Palermo, Amsterdam and London—all had to cope with growing numbers drawn into urban living by reviving economic activity. And with this recovery came monetary inflation, fueled by gold and silver from the mines of Europe, the goldfields of Africa, and, after the discoveries of Columbus, the treasure fleets of America. As population and monetary pressures are featured together in the same chapter, the discussion inevitably turns to the persistent rise in prices and then to the debates between Bodin and Malestroit over the quantitative effects of an increasing supply of bullion and, more recently, between Earl J. Hamilton and J. M. Keynes on the concept of profit inflation. The next three chapters deal in turn with agriculture, industry, and trade. Under agriculture Miskimin discusses the progress made by the different Western and Central European countries to cope with the rising demand for food: if real wages fell, it was due more to Malthusian pressures than profit inflation. Concerning industry he focuses on the expansion of cloth-making in response to the increasing need for clothing and on the metallurgy of Germany. And finally, his discussion of trade considers the failure of the Portuguese—in spite of their colonial drive—to establish a monopoly in the spice markets, the opening of the new lines to America, and the efforts of the other commercial nations of Europe to cope with the two great zones of deficit balances in the Baltic and Asia. The closing chapter takes up the problems of government and the ever-rising demand for revenue. When taxation could not meet the bill, the inevitable borrowing followed. It remains one of the paradoxes of the age that the resources of the Peninsula, Flanders, Italy, and the New World were not enough to save the monarchy in Spain from putting up the shutters of bankruptcy in 1557, 1575, 1596, and 1607.

The study concludes on a note of moderation, balancing material determinism against the role of institutions. Free trade was justified by natural law, but at the time it received a poor hearing, overshadowed by mercantilism, "essentially the history of failure." Miskimin has kept the study to a cutoff date in 1600 and so focuses on the epilogue to the medieval economy of Europe. But, by the same token, this is not an account of the "long sixteenth century," and it avoids the complex issues of the general crises of the seventeenth century that followed. That would make demands in excess of the allotted space: the emphasis of the book is how Europe grew in stature and struggled.

successfully, to cast off the dark days of the Black Death.

FRANK SPOONER
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JOSÉ ANTONIO FERRER BENIMELI. *Masonería, Iglesia e Ilustración: Un conflicto ideológico-político-religioso*. Volume 3, *Institucionalización del conflicto (1751-1800)*; volume 4, *La otra cara del conflicto. Conclusiones y bibliografía*. (Publicaciones de la Fundación Universitaria Española. Monografías, number 17.) Madrid: Fundación Universitaria Española. 1977. Pp. 725; 831.

In reviewing J. A. Ferrer Benimeli's first two volumes I concluded by noting that much more analysis and interpretation were needed to make the enormous amount of detail meaningful to the reader. Although these volumes, like the earlier ones, need some pruning of the masses of data, the author does venture more explicitly and at some length into explanation. As before, the range and quantity of the research is staggering: over six thousand separate items constitute the bibliography (about half of volume four), and this is broken down into useful categories. The appendixes which include important correspondence and lists of masons, make up over half of the work as a whole. One is reminded of the editing and collecting endeavors of nineteenth-century scholars, which made so much primary evidence easily available to subsequent historians. Particularly useful in this regard are pages 211-18 of volume four, where the author provides summary descriptions and conclusions encompassing the full work.

Ferrer Benimeli again discusses specific cases in some detail. Just as a specific situation in Florence had touched off Clement XII's antimasonic bull in 1739, an apparent threat of masonic subversion in Naples triggered Benedict XIV's bull against the lodges in 1751. Again, there were subtle jurisdictional disagreements between church and state, but Charles VII (the future Charles III of Spain) also revealed a profound antimason animus that carried over to his second reign—usually considered a classic example of enlightened despotism. In fact, during 1775-76 Charles pressured his son, Ferdinand IV of Naples, to renew the royal campaign against the masons. Although Ferdinand did heed his father's advice, his Austrian Habsburg wife continued to extend protection to the lodges. Ferrer Benimeli notes that most of the Habsburg offspring followed their father Francis, rather than their mother Maria Theresa, in their promason attitudes. Typically, Joseph II tried to bring all the lodges in his territories within one official structure in 1785.

Charles III never changed his view of the masons as a potentially subversive organization, dangerous to the state's security. Partly for this reason masonry did not appear in the peninsula until the next century. Ferrer Benimeli thus lays to rest the commonly held notion that the king promoted masonry and that many of his administrators belonged to lodges. His examination of Charles's edicts and letters, as well as similar evidence from other rulers and councillors effectively demonstrates both the hostility of the monarchs and the reasons for it. His analysis of papal and other ecclesiastical statements shows that the Church held similar views, while also fearing the masons' "heresy" as expressed in their commitment to religious toleration and an early form of ecumenicism.

Evidence from the lodges, however, reveals their basic conformity, a fact doubtless connected to their domination by nobility and clergy (most masters seem to have been from these ranks). Oaths and ceremonies clearly stated loyalty to king, country, and faith. And, although the masons favored religious toleration, such ideals were not widely bruited about: most masons worked quietly "within the system." Perhaps most tantalizing is the fact that many Catholic clergy continued to adhere to the lodges even after publication of the papal bulls condemning the order. Ferrer Benimeli argues that the large clerical component was attributable to the following: the appeal of mysterious ceremony, roughly paralleling liturgical aspects of Catholicism; the emphasis on good works of charity and philanthropy; and, perhaps most important, the fact that masonry emulated what was ethically best in Christianity. He also suggests that in many smaller cities and towns masonry filled the need for social diversion which in the great urban cultural centers was provided by courts, salons, and operas. Thus, despite the papal bulls (which in any case might be superseded by later popes), the lodges offered some clergy a means to participate modestly on the fringes of the Enlightenment.

Barruel and other contemporaries built the legend of the masons' contribution to the revolution. The order's secrecy and ceremonial, as well as its being confused with the Illuminati and with charlatans like Cagliostro, further contributed to the myth. The author observes that, in fact, by the later decades of the century the order was informally divided into spiritualist and rationalist wings (except in "solid" England apparently), but he does not explore the subject further. Interestingly, the revolutionary regimes also kept watch on the lodges, as evidence from Fouché's correspondence in the Directory years indicates. Most French lodges disappeared between 1791 and 1793 as their upper-class members scattered. But when

the order was later reconstituted on a middle- and lower-class basis, officials like Fouché approved its ideological content. The fact that some lodges survived as revolutionary clubs helped to reinforce counterrevolutionary views of them. All across the Continent lodges that re-emerged in the early nineteenth century became involved in liberal and nationalist causes, which indicates the profound transforming effect of 1789, both in ideas and social composition.

This is an important contribution to eighteenth-century studies despite the formidable mechanical flaws noted. Perhaps its meticulous author might consider a briefer general work on this subject, built on the impressive erudition displayed in these volumes.

PAUL J. HAUBEN
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J. E. FLOWER *et al.* *Writers and Politics in Modern Britain, France, and Germany*. New York: Holmes and Meier. 1977. Pp. vii, 109; vii, 78; vii, 94. \$17.50.

Writers and Politics in Modern Britain, France, and Germany is actually three separate volumes written by J. A. Morris, J. E. Flower, and C. E. Williams, who are respectively lecturer in English at Brunel University, professor of French literature at the University of Exeter, and lecturer in European Studies at the University of East Anglia. The section on Britain covers the period from 1880 to 1950, that on France from 1909 to 1961, and that on Germany from 1918 to 1945. Although it might have been a good idea to restrict all three sections to the twentieth century, relevant nineteenth-century background material on France and early twentieth-century writers and politics in the German linguistic area might have been included, thus achieving a more balanced treatment of the three countries.

The reader finds himself asking why each of the authors did not choose to write a separate volume, since there is certainly enough material available for more extensive and detailed treatment of the subject in each of the national literatures. Given the organizational integration of the three topics in this volume, the authors could have devoted more attention to an examination of the interrelationships among the three cultures, political systems, ideological currents, and literatures. This could most easily have been done in a more extensive conclusion. After all of the first-rate research and writing that obviously went into this book, it seems hardly fitting that its overall conclusions were stated so superficially, and in less than three pages.

The introduction, which discusses the inter-

action between politics and literature and rightly points out the vastness and complexities of attempting to delineate their relationship, possesses many of the qualities that should have been found in the book's conclusion. Flower, Morris, and Williams wisely point out that most writers who have dealt with the relationship between politics and literature have been more concerned with the ideas themselves than with how they have been expressed. In all three sections of the book, the authors succeed, each bringing the unique tools and perspective of his separate discipline to bear, in achieving this objective. Again, this reader kept hoping for an integration of the three separate areas of expertise in the analysis of the individual literatures of Britain, France, and Germany. Certainly there were writers and works in France from 1880 to 1909 and in Germany from 1880 to 1918 comparable to those in England from 1880 to 1909-18. Perhaps there ought to have been a fourth author of this volume—one with a background in comparative literature as well as politics.

The book is well written. The choice of writers and their works to support the authors' theses are generally good ones, although one cannot help but wonder why Williams chose to discuss *Mario the Magician* to the exclusion of other more profound and far-ranging works of Mann, such as *The Magic Mountain*. Such questions detract but little from the obvious fact that this volume represents a serious and generally successful effort to chart new paths in the tangled forest of the relationship of politics to art.

THOMAS G. DUFFY
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DONATELLA BOLECH CECCHI. *L'accordo di due Imperi: L'accordo italo-inglese del 16 aprile 1938*. (Quaderni della rivista "Il Politico," number 16.) Milan: A. Giuffrè, for Istituto di Scienze Politiche dell'Università di Pavia. 1977. Pp. 300.

In the spring and early autumn of 1938, the British and Italian governments signed two key international treaties. They were the Anglo-Italian Agreement of April 16, which was intended to resolve outstanding European and colonial problems between London and Rome, and the four-power Munich pact of September 30, which among other things forestalled the outbreak of general hostilities. Of these two treaties, the latter has been the focus of major and continuous attention. On the other hand, the Anglo-Italian Agreement, which Mussolini dubbed "l'accordo di due Imperi," has in the main been treated in peripheral or secondary fashion. Has this disparity in treatment been warranted? Yes, to the extent that the

Munich settlement was the far more fateful and dramatic of the two treaties. No, if one considers that the Agreement of April 16, which further renewed vital lines of communication between Downing Street and the Palazzo Venezia, helped make the Munich conference possible. Indeed, for better or worse, in the absence of the agreement the Second World War might have begun a year earlier. As the author of this well-written and carefully organized study indicates, the agreement provides a first-rate backdrop as well as a unique and meaningful framework (that is, Anglo-Italian relations) for understanding and measuring the effects of various entangling and persistent feelings of anxiety, ambition, optimism, and disgust both within and between the states of the Entente Cordiale and the Axis. It also helped to heighten such emotions during the developing and climactic weeks and days of the Sudetenland crisis. Moreover, the origins, provisions, and impact of the "Easter Pact" or "l'accordo di Pasqua" of 1938, as the agreement was also called, offer a better test-case for comprehending the reasons for the failure of Chamberlain's appeasement policy than the coerced and soon-aborted Munich treaty.

To be sure, Donatella Bolech Cecchi's skillful handling of these and other points associated with the agreement makes this study significant. Yet, although the book has important merits, it also has certain weaknesses that must be noted. In the first place, the study ends too abruptly in mid-November 1938, the moment at which the agreement actually came into operation. A concluding chapter covering Chamberlain's pending visit to Rome, if not Italy's invasion of Albania, would have presented a more complete picture of the accord. Second, this study is based substantially upon British documentary materials in the Public Record Office. German, French, and Portuguese collections have been used less extensively, while the Belgian records, as well as the microfilm materials of the National Archives in Washington, D.C., have been passed over. Moreover, although the published memoirs and in some cases the private papers of such leading individuals as Cadogan, Ciano, Craneborne, François-Poncet, Henderson, and Magistrati have been used with good effect, the Chamberlain papers as well as those of Lord Perth, who was the British ambassador in Rome in 1938, have apparently been ignored. Furthermore, there is nothing new here from the Italian state archives (due probably to no fault on the author's part!). And third, because of the heavy reliance upon British sources, this account of the agreement is at times unbalanced. Events, attitudes, and actions occasionally have a decidedly British, as opposed to Anglo-Italian or international, flavor and perspective.

It must be emphasized that this study was never intended as a definitive work on the Agreement of April 16. That being so, it would seem appropriate to assert that not only do its strengths outweigh its weaknesses but also it is a valuable contribution to the historical literature dealing with the last full year of peace.

WILLIAM D. BRIGGS
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HEINZ GOLLWITZER, editor. *Europäische Bauernparteien im 20. Jahrhundert*. (Quellen und Forschungen zur Agrargeschichte, number 29.) New York: Gustav Fischer Verlag. 1977. Pp. 702.

This collection of studies on European peasant parties has been arranged by regions: Scandinavia and Finland, East-Central and Southern Europe, Central and Western Europe. It also includes a separate essay on the International Peasant Union (the Green International) and a synthesis, "Peasant Democracy in the 20th Century," written by Heinz Gollwitzer, editor of the volume. Each chapter of the book is supplied with bibliographies of relevant publications, including American ones. The book was written with a visible concern for clear structure, awareness of basic issues, respect for detail, and attention to scholarly apparatus. It constitutes volume twenty-nine of the noted series *Quellen und Forschungen zur Agrargeschichte*.

This remarkable piece of social history presents peasant parties in Europe against the broader background of peasant movements in general from their beginnings in the nineteenth century. The book demonstrates a full realization of the role of peasant movements in the national and cultural awakening of the masses and in their liberation from the remnants of feudalism, political oppression, social injustice, and general backwardness. Thus, the early political activities of peasants, their concern for education, the various programs of their organizations, the cooperative movement, and the international associations of farmers have been assessed and related to the organization and transformation of peasant parties and the political activities of peasants. A careful comparison of European peasant movements with the Russian *narodniki*, as well as with American populists, broadens the scope of the book and deepens the understanding of movements that started as a class activity and developed into national parties, responsible for governments in their countries. The authors of these studies carefully consider the differences among peasant parties, reasons why their growth differed in Western and East-Central Europe, and their international cooperation. The study covers the whole period until after World

War II, when peasant parties were forcibly dissipated in the "patriotic fronts" of the "dictatorship of the proletariat."

The book is a pioneering enterprise since no similar study has been published. The Polish "Encyclopedia of the Peasant Movement," compiled in Warsaw last year, perhaps could be placed close to this study, had the Warsaw government not denied permission for publication. This work puts in high relief the place of peasant parties in building and defending parliamentary democracies in Europe. Even their failures, their persecution by authoritarian regimes, and their suppression by the Nazi and Soviet invasions did not stop the advance of peasant political activism. These parties induced the peasants to participate in the anti-Nazi resistance movement and led nations of Eastern and Central Europe in vain struggles for free elections after World War II. Their representatives, "the brothers Antun and Stjepan Radić as well as Maček in Yugoslavia, Švehla and Hodža in Czechoslovakia, Witos in Poland or Maniu in Romania, showed not only experience and high intellectual skills in their conduct, but also the moral integrity that made of them 'charismatic leaders' among the peasants" (p. 672).

A political force, more pragmatic and experimental than ideological, with its own ideology called agrarianism, the peasant parties were devoted to "peace, land, and peasant labor; they supported only such governments as were ready to preserve the inner and external peace" (p. 678). But they were always determined "to protect land, ownership, family, and nation." A constructive force of the middle way, the peasant parties can be placed somewhere between capitalist-liberals on one hand and planned economy and proletarian dictatorship on the other. Open to Christian influences and social teaching, they were considered as a third way that promised a "green uprising," leading toward a more human state and society and arousing expectations even among writers such as G. K. Chesterton.

This complete and thorough survey of peasant movements is unquestionably useful and valuable. But even a valuable work contains some omissions or misunderstandings that should be noted for correction in the future. The constant use of the term "succession states" with reference to the states created after World War I implies a biased attitude toward over one thousand years of historical existence in those states that earlier had succumbed to imperialistic neighbors but were reborn in 1918. The post-World War II *Manifesto to the Peasants of Asia* is not mentioned, although it indicated new, important perspectives. Calling the Polish regime after 1926 "dictatorial" is a surrender to the rhetoric of political polemics rather than

an expression of reality. In this connection the proportionality of elections as a cause of the proliferation of political parties in all of the parliaments of East-Central Europe, except Czechoslovakia, seems to be insufficiently emphasized as one of many causes of weakness in the executive branches of government. In spite of unusual success in spelling the names from various orthographic systems and ethnic particularities, misspellings have appeared here and there and could have been avoided. A subject index would have made this book a more handy basic reference tool.

But these are insignificant imperfections in a work that will stand the test of time. In a world where peasants constitute a majority and democracy needs constant support, this excellent analysis of the peasant tradition provides readers with a better understanding of this potential for change that looks so relevant in its historical dimensions.

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RONALD EDWARD ZUPKO. *British Weights and Measures: A History from Antiquity to the Seventeenth Century*. Madison: University of Wisconsin Press. 1977. Pp. xvi, 248. \$15.00.

The economic elements of justice and lawgiving have never been far from the surface of religious preoccupations. No holy book is devoid of biblical-style precepts enjoining government to regulate weights and measures. Mankind's earliest surviving written records seem to be divided with equal impartiality between myth-bearing prose and taxation lists. Yet the historian's problem starts at precisely this point. Units of measurement are essentially artificial, valid only so far as they are agreed to by all, easily evaded by cheats, always the subject of friction on the borders of different governmental or ethnic areas. The endless different ways in which the same goods can be viewed, measured, and divided (for example, length, weight, volume capacity, and density), added to the apparently basic human tendency to seek advantage where there may be ambiguity, have always made the administrators' task thankless, petty, and ultimately frustrated. Moreover, the passionate addiction to the excellence of local peculiarities, perhaps a constant of human behavior, in the imperfect market situation of pre-industrial Europe, added to the problem. The historian may indeed be surprised to learn from Ronald Edward Zupko that some degree of uniformity was achieved amidst the flourishing of a multiplicity of indigenous variations on a theme. Zupko, like some metrical Ariadne, has already provided in his

Dictionary of English Weights and Measures a guide through the labyrinth for those anxious historians who fear that their misidentification of the precise modern, or contemporary, equivalent of historical measures may lead them to build a beguiling will-o-the-wisp instead of a solid hypothesis. His latest work greatly adds to our indebtedness in this regard. His exhaustive thoroughness will make it a classic work of reference. Half the book is devoted to appendixes which provide a comprehensive listing of the weights and measures of merchandise in the English import and export trade between 1500 and 1800, British pre-imperial units, imperial units, and premetric weights and measures in Western and Eastern Europe, together with an inclusive bibliography of source material. This section will be an invaluable reference source for all economic historians.

The most fascinating section, however, is the first hundred pages in which Zupko painstakingly pieces together literature, statute law, archeological finds, and historical relics to establish the difficult history of the standardization of measurement and its implementation in Britain. The problem of finding the official to enforce the law, continuously, impartially, and without fear or favor, was repetitious, tedious, and, in the final analysis, impossible, even if accurate physical standard measures could be made and maintained in all the areas of the country. In the long run, even an archbishop or the chancellor of Oxford University succumbed to the greater pressure of "political, mercantile, industrial, military or religious interests." Zupko does not pursue these byways, but his attempt to weave the history of standardization into the integral cloth of the economic development of Europe suggests interesting new lines of thought.

SYBIL M. JACK
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J. A. GUY. *The Cardinal's Court: The Impact of Thomas Wolsey in Star Chamber*. Totowa, N. J.: Rowman and Littlefield. 1977. Pp. x, 191. \$15.00.

Cardinal Wolsey's reputation has been in eclipse ever since A. F. Pollard judged him harshly fifty years ago. While J. J. Scarisbrick successfully rehabilitated Wolsey's foreign policy, showing the cardinal to have been under the influence of humanist ideals of peace, efforts to gain a fresh perspective on Wolsey as a judge, administrator, and maker of domestic policy have been less successful. This has been the result of G. R. Elton's depiction of Wolsey as only a gifted amateur in law and as a chief minister without the passion for order essential to wide-ranging and enduring achievements in

the reform of government. The great enlargement of activity in the court of chancery and in the exercise of the council's public jurisdiction in Star Chamber under Wolsey have been variously ascribed to mere evolutionary pressures in society (Pollard) and to the avarice, arrogance, and political schemes which were the essence of Wolsey's ministerial character. Only Wolsey's "antifeudal" policy and his advancement of the "unity of the state" drew much praise from leading Henrician scholars.

Now, J. A. Guy has recast his excellent Cambridge dissertation into an even better book. And, in doing so, on the basis of a brilliant reconstruction of the chaotic and scattered "Star Chamber Archive" for the period 1515-29, Guy has made clear the need for a major revision of all earlier accounts of Wolsey's domestic policy and impact on the machinery of government.

Indeed, in three important chapters of the present work, Guy has shown the way. By carefully separating "Myth and Reality" (pp. 1-21), through a useful study of Wolsey's management of the council (pp. 23-50), and in a concluding section on Wolsey's achievement in Star Chamber, he has clearly shown that it was under Wolsey that the conciliar court matured as an institution. It only remained for Wolsey's servant and successor Thomas Cromwell to complete the development of the court "after the shape of Wolsey's blue-print."

How the court took shape under the pressure of a tremendous rise in case load is made clear in the two central chapters (pp. 51-117). These provide a masterly exposition of the organization of the court, its personnel, the matters heard, the prominence of arbitration and compromise in Wolsey's thinking, means of enforcement, problems of procedures, and relations with the courts of common law and the "undercourts" created by conciliar commissions. The reader is constantly impressed with Wolsey's initiative, drive, concern for equity, tough handling of official corruption, and sympathy for the poor and oppressed. It is also clear that Wolsey's eagerness to meet plaintiffs' needs for equitable remedies put defendants at a disadvantage because of a lack of procedural rigor. Frivolous suits prospered, and resort was had from judgments already given elsewhere.

But Guy's close analysis of 482 cases in a more general study of the 1,034 definite ones during Wolsey's presidency convinces me this was a small price to pay. After all, the judicial work of the council revived. There was a coherent policy of enforcement through cooperation with the courts of common law. Wolsey was a flexible judge whose chief weakness was a naive trust—in suitors and officials who often took advantage of generous impulses to chip away at the cardinal's commitment

to fair play. This was not unique in Wolsey's court, however. Many achievements were.

ARTHUR J. SLAVIN
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JULIAN CORNWALL. *Revolt of the Peasantry, 1549*. Boston: Routledge and Kegan Paul. 1978. Pp. 254. \$14.25.

Popular rebellion was as much a part of sixteenth-century England as the divorces of Henry VIII and the ill-fated romances of his daughter. Each of the Tudor sovereigns faced at least one major rising and countless lesser disturbances and conspiracies, but the most menacing rebellion of the century occurred in 1549 when more than twenty counties seethed with violent social and religious protests. Traditionally seen as two separate revolts, the rebellions of 1549 had much in common. Everywhere the upper gentry and town oligarchs encountered vigorous popular opposition. The commons, loyal to Edward VI, first sought local remedies for their grievances. In most areas leading gentry were unsympathetic to the policies of the central government and therefore lacked the determination necessary to maintain law and order. Conflict between the court and country ensued as local communities divided among themselves in an attempt to thwart the reforms of Protector Somerset's government.

Rather than pursue the complex interaction between local communities and the government, Julian Cornwall has written a narrative stressing that the rebellions of 1549 had two distinct geographical focuses—one in the west, the other in Norfolk—and few similarities. He finds the Western Rebellion more important, because the commons "fought for what they believed in, religion and racial identity." Robert Kett's men, encamped outside Norwich on Mousehold Heath, "had no such inspiration"; the author concludes that "men will die for an ideal but not for material interests."

Although Cornwall offers a readable account that will be serviceable to general readers unfamiliar with the works of Frances Rose-Troup, F. W. Russell, and S. T. Bindoff, specialists will find the book unsatisfactory. The fact that the volume contains only nineteen footnotes reveals the author's heavy dependence on the older secondary sources and makes it very difficult to determine the authorities from which conclusions are drawn. The bibliography omits important recent publications by Hoak, Manning, Heinze, and others as well as American dissertations on Richard, Lord Rich, and John Russell, Earl of Bedford.

By focusing almost exclusively on the Western Rebellion and Kett's Rebellion, Cornwall seri-

ously underestimates the extensive disorder in other counties, especially Suffolk, Hampshire, Oxfordshire, and Kent. The characterization of the rebellions as a revolt of the peasantry oversimplifies risings in which artisans and the urban poor played a major role and neglects the contribution of the lesser gentry in Cornwall. Factual errors also mar the book's utility; for example, in 1543 Robert Kett was assessed on goods worth £60, not £160. Moreover, the assertion that Somerset appointed Northampton to command the first army sent against Kett because the protector perceived Warwick as a dangerous rival rests on no reputable documentary authority. Cornwall's most valuable contribution lies in his meticulous reconstruction of the military campaigns that led to the pacification of Norfolk and the west. Some scholars may accept his emphasis on religious conflict in Devon and Cornwall, but others, less influenced by Rose-Troup, will want to consider the suggestion of Pollard and Fletcher that social conflict, especially antigentry sentiment, was also significant.

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STEPHEN K. LAND. *Kett's Rebellion: The Norfolk Rising of 1549*. Totowa, N.J.: Rowman and Littlefield. 1978. Pp. 165. \$15.00.

Stephen K. Land has written a short, engaging book. The author's purpose is to retell the events of the rebellion, augmented by twentieth-century scholarship on its background and origins. Land's instincts are those of the local historian. He brings the reader close to the topography, the architecture, some representative families, and the particular economic grievances of the Norfolk region, as well as to the daily unfolding of the rebellion itself. This is a refreshing change from the court-policy-centered narratives of A. F. Pollard, W. K. Jordan, and M. L. Bush, upon whom the author nevertheless acknowledges his dependence. He discusses the essentially conservative, anti-capitalist aspirations of the Norfolk rebels, their enthusiasm for Protector Somerset's policies, and their anger toward the local gentry who seemed to be undermining customary peasant privileges. He also stresses the power vacuum in Norfolk due to the attainder of Thomas Howard, Duke of Norfolk, in 1547 and the subsequent squabbling among the lesser families. None of this is new—it is dealt with incisively in S. T. Bindoff's pamphlet, *Kett's Rebellion* (1949), and in Anthony Fletcher's *Tudor Rebellions* (1973)—but Land embellishes on the issues valuably.

The author is less successful in linking the Nor-

folk uprising with the larger national currents of the period. He frames most of his narrative between elementary chapters on Somerset's rise and fall. More disappointing is Land's treatment of the possible social and intellectual influences on the rebellion. Land should have confronted a number of difficult issues: to what degree and why elements of the Norwich population joined Kett's movement; whether the poor law of 1547, which authorized slavery as punishment for the refusal to work, was an issue in the disturbances; and whether Commonwealth thought influenced popular aspirations in 1549. He deals only with the first of these questions, and at that, within one paragraph (p. 53). One would expect a more searching discussion, especially since Land treats the reader to two pages on the ill-fated romance of the Norfolk belle, Amy Rosbart, and the Earl of Warwick's son, Robert Dudley (pp. 127, 129).

Three maps, seven genealogical tables, a bibliography, and an adequate index are provided. Tudor specialists will regret the slim documentation of the book and question the absence from the bibliography of such important studies as Joan Thirsk, ed., *The Agrarian History of England and Wales*, vol. 4 (1967); Whitney R. D. Jones, *The Tudor Commonwealth 1529-1559* (1970); and Peter Clark and Paul Slack, eds., *Crisis and Order in English Towns 1500-1700* (1972). There is a great deal more social and intellectual context setting for Kett's Rebellion than Land has attempted to demonstrate. Yet, his book has much to recommend it to the general reader, students of rebellion, and Norfolk local history enthusiasts.

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ADRIAN MOREY. *The Catholic Subjects of Elizabeth I*. Totowa, N.J.: Rowman and Littlefield. 1978. Pp. 240. \$16.50.

Written in a readable style, this book outlines the history of English Catholicism from the attempts of Queen Mary and Reginald Pole to restore papal authority in 1554 to the Catholic rejection of, and struggle against, the Elizabethan settlement of 1559. Despite valiant efforts of clergy and lay people, Catholicism was by the end of the Tudor period merely a minority religion sustained and protected in aristocratic households.

A political compromise, the Elizabethan church was too Protestant for the queen and for a majority of her lay and clerical subjects. Even so, most beneficed clergy accepted the settlement, which was of great advantage to the government, Adrian Morey maintains, since the scarcity of priests left Catholics almost completely dependent upon the state church for spiritual comfort.

Without domestic or foreign leadership Catholics suffered a "steady attrition," especially among the lower classes, because of both the costs of recusancy and the unresolved dispute about the legitimacy of attending state churches. Widespread persecution began when the government reacted harshly to Catholic responses, particularly the founding of William Allen's college at Douai in 1568 to train English Catholic priests and Pius V's controversial bull of excommunication and deposition, issued in 1570 after the first uprising to win English succession rights for the then imprisoned Mary, Queen of Scots.

In his analysis of the Catholics' effort to reconvert England, Morey astutely focuses on their diversity and dissension. The nonconforming English (Marian) clergy distrusted the aid of the Counter Reformation, viewing the young missionary priests who returned to England from Allen's school as parvenus who would try to supplant the Sarum Use with the Roman Rite. Envied and feared by all were the Jesuits, who, although small in number, inspired Catholics everywhere with the martyrdom of Edmund Campion.

The chapter on the English exiles and their impact on education is particularly informative. William Allen's school, bravely struggling against "financial stringency," educated not only missionary priests but also lay Catholic boys. The material on women's education is also very welcome despite its paucity; historians of the Catholic and Protestant Reformations increasingly have recognized the importance of women to these movements.

As more and more priests arrived in England, persecution intensified. Morey offers some interesting biographical sketches of three laymen whose religious faith led to imprisonment, fines, and exclusion from office. Examples of how they and others were able to survive as Catholics by avoiding Anglican baptism, marriage rites, and even recusancy fines lend some humor to this description of tragic human suffering.

Based primarily on published material, this book merits attention because of its objectivity and insight into the problems of the Catholic minority in Elizabethan England. Although Morey refrains from lecturing about the loss of human rights, his work ends with pathos, for after the death of the queen, Catholics turned in desperation to violence against her successor only to suffer renewed harassment and death.

RETHA M. WARNICKE
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PAUL CHRISTIANSON. *Reformers and Babylon: English Apocalyptic Visions from the Reformation to the Eve of the Civil War*. Buffalo, N.Y.: University of Toronto Press. 1978. Pp. x, 285. \$17.50.

Although Puritan millennialism has been the subject of much scholarly research in recent years (see the works of Tai Liu, B. S. Capp, and Willie Lamont), the generic subject of Protestant apocalypticism, of which Puritan millennialism is only a species, is now getting more attention in the history of English religious thought (see the work of Bryan Ball). The volume under review is a case in point. According to Paul Christianson, apocalyptic thought is characterized by "a polarized view of the universe, a catastrophic explanation of events, and a firm concern with prophecy and its fulfillment" (p. 3) whereas millennial thought is characterized by "a literal thousand-year reign of the saints on earth" (p. 7). Using these definitions, Christianson begins his study with John Ball, the first spokesman for a century of Protestant apocalyptic writers beginning at the Henrician schism, and terminates it in 1642—before the full flowering of Puritan millennial thought.

For many Anglican and moderate Puritan apocalyptic writers, according to Christianson, John Ball provided a historical paradigm, based upon the biblical books of *Daniel* and *Revelation*, when he identified Babylon with Rome and the pope with antichrist. Separatists, however, regarded the Anglican Church, too, as a part of Babylon and the supreme governor as a minion of antichrist, while Arminians, who stressed episcopacy *jure divino*, rejected Ball's paradigm altogether. Radical Puritans sought Christ's new Jerusalem either through changes in Anglican Church government or by "root and branch" abolition. One must wonder just how much this Protestant diversity was shaped by, or itself shaped, these various strands of apocalyptic thought. But Christianson has clearly shown that by 1642 some "divines and laymen chose to interpret their quarrel with the royalists in apocalyptic terms" (p. 241).

There is something of a contradiction in Christianson's treatment of the political impact of apocalypticism. In his introduction, for example, he suggests that it "displayed neither revolutionary nor conservative characteristics in its social and political thrust" (p. 7). Yet, throughout the course of his book he carefully delineates two contrasting attitudes. Some writers, such as John Jewel, John Aylmer, Edwin Sandys (all bishops), Thomas Brightman, and Alexander Leighton, relied on the godly magistrate, whether princes or parliaments, to bring about reform. Other writers, such as Robert Browne, Robert Harrison (both Separatists), John Lilburne, William Prynne, John Bastwick, and Jeremiah Burroughs (all radical Puritans), relied on the persecuted and oppressed to bring about change. John Foxe, the martyrologist, Joseph Mede, the first millennialist, and Henry Burton, who switched, drew from both political traditions. There may well have been a third

apocalyptic political alternative—something of a theocratic counterpart to the Arminian elevation of the godly bishop—which called for the godly elect to rule the country. But that political alternative became an integral part of the Puritan millennialism which bloomed in the 1640s and 1650s.

Whether, as Christianson contends, "the apocalyptic tradition constituted a crucial part of the world view through which all of these men scrutinized the politics of church and state" (p. 138), or whether it was the reverse of this proposition, he has added an important dimension to the growing typology of English Protestant thought. And, particularly, he has given us another link in the chain, already well begun by Murray J. Tolmie (*The Triumph of the Saints: The Separate Churches, 1616–1649* [1977]), connecting the Levellers with the Separatist churches. Finally, an appendix makes a strong case for Burroughs as the author of *A Glimpse of Sions Glory* (1641), an important tract that has been attributed for some time to Thomas Goodwin.

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ROY SHERWOOD. *The Court of Oliver Cromwell*. Totowa, N.J.: Rowman and Littlefield. 1978. Pp. 194. \$14.50.

A careful description of the organization of Cromwell's household and the identification of the main officers are the principal contributions of this book. It seems unlikely that much can be added to the structure of the Protector's court. Roy Sherwood's success in piecing together the evidence is considerable, because a basic source for such a study, the account books, is missing. In their absence Sherwood has had recourse to the state papers, to newspapers, and to the lists of persons who participated in the planning for, or who marched in the procession at, Cromwell's funeral.

After a preliminary account of the residences utilized by the Protector, succeeding chapters treat the departments of the household, including that below stairs, the stables, the chamber, and the wardrobe. A description of court life follows, and a conclusion summarizes the main findings.

The contrast between Cromwell's court and those of earlier monarchs was striking. Only Whitehall and Hampton Court seem to have been occupied regularly, as compared to the numerous palaces used earlier. The household staffs were smaller and the costs, in proportion to all government expenditure, were substantially lower. There was less ostentation, though court life was conducted with gravity. It is clear from royalist correspondence not used by Sherwood that Cromwell

was sensitive to the importance of preserving the dignity of his position. Even before his inauguration, Cromwell is said to have rebuked one who failed to uncover, and shortly after he moved to Whitehall it was asserted that it was ten times more trouble to speak to him than to any king in former times (*Calendar of the Clarendon State Papers*, II [1869]: 208, 379).

Sherwood shows that the old principles of patronage and purchase had little influence on the household. Office in Cromwell's court was not a pathway to riches. The principal officers were politicians as well, but at the lower level there was little sign of partisanship. In fact the author shows that there were a few holdovers from the royal regime, just as a few of Cromwell's musicians managed to secure positions in the court of Charles II. Others were unsuccessful in their applications for continuance after the Restoration, though Sherwood's apprehension (p. 133) that the Kinnersley tradition in the wardrobe was broken deserves reconsideration, because a list of 1663 shows Philip Kinnersley serving in that department (*Bulletin of the Institute of Historical Research*, 19 [1942-43]: 19).

One conclusion, that Cromwell's court reflected rather than set patterns for society, is difficult to prove conclusively, if only because of its short life. As a general statement this may be true, but in 1655 a royalist lady sent the pattern of some garment to her sister, "as it is worne at Court" (*English Historical Review*, 47 [1932]: 311). Whatever the case in this one respect, there is no doubt that the author of this book has conclusively confirmed that majesty befitted Cromwell and that he fittingly filled his exalted rank.

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CHRISTOPHER HILL. *Milton and the English Revolution*. New York: Viking Press. 1978. Pp. xviii, 541. \$20.00.

Christopher Hill's aim is to rescue Milton from the Miltonists. Christian orthodoxy and scholastic criticism, Hill argues, have obscured the real Milton, the poet of the English Revolution. The book contains a biographical account placing the poet and his works in historical context, an analysis of Milton's "Christian Doctrine," and a reading of the major poems that stresses contextual themes. There have been earlier attempts to explore Milton, the Puritan—by D. M. Wolfe, William Haller, and many others—but never as uncompromisingly by anyone with Hill's mastery of English radical traditions. Hill points to a Milton totally engaged in the conflict of three cultures: the

traditional monarchist one that he attacked, and the orthodox Calvinist and "popular heretical" ones that he uneasily straddled. The discussion of the third culture repeats themes from Hill's other books but is again brilliantly done. Throughout, Hill brings to bear all his immense learning in the intellectual milieu of the seventeenth century. Under "Christian Doctrine" we are guided through the ambiguities of the antithesis of liberty and discipline, Arminian reactions to predestinarian Calvinism, millenarianism, mortalism, and much else. Hill always insists that the intellectual ferment of Milton's own time had even more of an impact on him than his extensive reading in the classical philosophers and the Christian Fathers.

There are obvious hazards in so sharp a thesis, even for a historian of Hill's authority. He often stresses Milton's "permanent dialogue" with the radicals, yet he never quite documents it convincingly. Many of Milton's ideas clearly had "affinities" with those of the radical Left. But it takes two to make a dialogue. A few approving quotations do not make one—and those date mainly from the 1650s rather than the previous decade when dialogue might have been more fruitful. This raises the question of Milton's influence before his emergence as champion of the Commonwealth. Hill disputes W. R. Parker's view that Milton was relatively obscure until 1649, wondering why, if this were so, the republic invited him to serve as propagandist and Latin Secretary. Yet apart from the divorce pamphlets, there is little sign that Milton was widely known outside a narrow circle of intellectuals and radical politicians. Milton had friends in high places—Vane, Bradshaw, Wentworth—who knew his talents even if few others did. Hill concedes Milton's earlier failure to reach a mass audience when he notes Milton's more direct style in 1659-60, compared with the "elaborately orchestrated" pamphlets of the 1640s.

Hill almost always downplays traditional influences. Granted, such influences as Platonism and Christian humanism have been overstated and the balance needs redressing. Yet the Milton who defended the ancients and classical forms was perhaps less committed to progressive ideas than the book sometimes implies. The context is also incomplete, in that the conservative social forces that in the end defeated the revolution receive little attention. Hill is right to remind us of popular hostility to the old order. But we might also be reminded of the continuities of rural life, which frustrated others besides Milton who cherished dreams of teaching liberty to the people.

But if Hill too easily relates Milton to a "middle-class culture," which has taken some hard knocks from historians, he still provides a superb account of the poet whose mission was "to speak to and for

the English nation" (p. 39). He does not conceal the limits of Milton's radicalism—on the rights of women, for example—or his decline from the fearless spokesman for liberty into the author of the "pitiful oligarchic proposals" of 1659–60. Some readers may gulp at advice to "think of the Ranters when reading *Samson Agonistes*" (p. 314) or at alleged echoes of the English Civil War in the War in Heaven. Yet this is a magnificent reading of the great poems, packed with insights that only a historian of Hill's profundity can suggest. One may query specific parallels and still agree that the great poems were indeed products of the disaster of 1660, messages for God's people about "the failed revolution, the millennium that did not come" (p. 362). Milton wrote in an epic tradition on universal themes, but with particular resonance for the "fit though few" who had shared the agonies of his times.

Everyone who has read Milton—and in spite of F. R. Leavis this ought to mean any cultivated person in the English-speaking world—and who has the slightest interest in the seventeenth century, should read this book. They will find much to admire, much to question: they will always be challenged and stimulated. There are other approaches to Milton, but in the future no one can deny the value of reading him as a poet engulfed by a revolution.

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J. R. JACOB. *Robert Boyle and the English Revolution: A Study in Social and Intellectual Change*. (Studies in the History of Science.) New York: Burt Franklin. 1978. Pp. 240. \$18.95.

Few scientists of the seventeenth century match Robert Boyle for contemporary influence and posthumous fame. Yet Boyle's voluminous published works, not insubstantial correspondence, and unpublished papers have attracted remarkably little critical scholarship. As J. R. Jacob rightly points out, the unending flow of papers and books on Boyle constitutes only a minor and specialized contribution toward our understanding of this formidable figure.

Jacob is concerned to produce a historically more meaningful portrait of Boyle, by first relating Boyle's thought to his experience of the drastic changes in affairs during the revolutionary period, and second, by evolving a synthesis of the "social with the religious, philosophical, and technical aspects of his biography." This is a challenging and thoroughly defensible conception of biography, and one that has been applied only with the most limited success to scientific intellectuals.

The span of somewhat less than one hundred and eighty pages of text available to Jacob clearly precludes the application of his ambitious program to the whole biography of the long-lived Boyle. It should perhaps have been made clearer that this study concentrates largely on Boyle's early life. Indeed, the longest part of the text deals with Boyle's intellectual development before the age of twenty-five. Toward the end of the book Jacob discusses some ten years of the Interregnum in about twenty pages; and there follows a short essay on Boyle at the time of the Restoration. Thus fifty years of Boyle's life receive very scant coverage. The period of his most active literary productivity is no doubt reserved for comment in a later work.

The primary interest and originality of this book, therefore, lie in its study of the intellectual development of the young Boyle. Jacob is the first scholar to attempt a reasoned exegesis of the juvenilia. These include unpublished ethical writings, some short essays, the relatively obscure and very poorly regarded romances *Seraphic Love* and the *Martyrdom of Theodora*, and the slightly better-known *Considerations Touching the Style of the Holy Scriptures*. The author draws a sympathetic portrait of Boyle as a devout young aristocrat, "fighting to make his way in the world against the blows dealt him by the civil war," reaching toward a "new kind of piety," and a new comprehension of divine providence. Although Jacob presents Boyle as an innovator, he fully acknowledges Boyle's debt to a wide circle of intellectual reformers, ranging from Falkland and Wotton, to Andreae and Comenius. Curiously, Jacob does not recognize the influence that the writings of Francis Bacon exerted over Boyle. Gassendi and Descartes do not even merit mention in the index. But Jacob fully appreciates the role played by Samuel Hartlib and his friends at an important formative phase of Boyle's life.

Although correct in general terms, some of Jacob's major findings are open to criticism. For instance, he suggests that the utopian tract *Maecaria* (1641) provided an important model for Boyle's thinking. It is notable, however, that *Maecaria* was written very much with the circumstances of 1641–42 in mind, and there is no evidence from any source that the tract was cited thereafter or used even by the Hartlib circle. Equally, although *Via Lucis*, to which Jacob attaches importance in relation to Boyle, was completed by Comenius during his visit to England, there is no evidence that the manuscript was in Hartlib's possession after Comenius's return to Europe in 1642. It is scarcely plausible that in England only Boyle had access to this document during the long intervening period between its composition and publication in 1668. Jacob sug-

gests further that the Invisible College was essentially a creation of Samuel Hartlib, who then introduced Boyle into its ranks, the college thereby becoming Boyle's first important venture into science and social affairs. This hypothesis tends to push back Boyle's relationship with the Hartlib circle to a date well before that suggested by the evidence; it obscures the fact that it was Boyle who introduced Hartlib to the Invisible College; and, finally, it ignores the important role played by Benjamin Worsley in the affairs of the college. It is a major lapse to disregard the relationship between Worsley and Boyle. Jacob mentions Worsley on only one occasion, in the context of events as late as 1657. Finally, the evidence adduced by the author to justify the identification of Boyle's "little dialogue" of 1647 with the important "Invitation to a free and generous communication of Secrets," published in 1655, is not at all convincing. It is also questionable whether this essay should be regarded as Boyle's reaction against the aristocratic "Cult of Curiosities." The contextual evidence suggests that he and Hartlib were much more preoccupied with undermining the habitual tendency to secrecy among craftsmen and medical practitioners.

The above points illustrate the pitfalls facing the biographer operating in the context chosen by Jacob. The reader will be sympathetic to the general trend of the argument, but the attempted synthesis lacks sufficient precision to be entirely convincing. Perhaps the main reservation that readers will entertain about the present biography is the very small part played in it by analyses of Boyle's unpublished and published writings on scientific subjects produced during his formative years. Boyle's genuinely gifted and far-ranging work as an experimentalist and the carefully evolved "corpuscular philosophy"—his most enduring contribution to natural philosophy—remain in the background, as if their relationship with Boyle's ethical outlook were either proven or obvious. This is not the case, and it is unfortunate that these vitally important dimensions of Boyle's early life have not received the prominence that they merit. The reader will look forward to much fuller exposition of these matters in later writings by Jacob.

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I. M. GREEN. *The Re-Establishment of the Church of England, 1660-1663*. (Oxford Historical Monographs.) New York: Oxford University Press. 1978. Pp. x, 263. \$20.50.

There has been a longstanding generalization in Stuart studies that in the Restoration of 1660 reac-

tion against the Puritans was unsparing and instantaneous. The ground swell of victorious Anglicanism swept away all vestige of Presbyterian-Congregational infiltration, restoring the Book of Common Prayer and a strong episcopalian hierarchy. As proof, what is better than the great ejection of 1662 and the Clarendon Code? This rigid interpretation, remnant of Macaulayan history, stood so well because contemporaries, Richard Baxter and Clarendon especially, vouched for it. Besides, it was easy to teach.

Modern works reinforced the theme of Laudian resurgence with R. S. Bosher's *The Making of the Restoration Settlement: the Influence of the Laudians* (1951) and the *Hibbert Journal's* tercentenary articles, under the general title, "The Ejection and its Consequences, 1662-1962," by L. A. Garrard, *et al.* But then came studies by A. O. Whiteman and Douglas Lacey suggesting that numerous Puritans had remained in the woodwork of the Anglican Church and in parliament, despite attempts at uniformity and political exclusion. Lacey suggested that a sort of neo-Puritanism lay just below the surface in Restoration politics so that the Revolution of 1688 and the Toleration Act might be regarded as belated Puritan triumphs.

Now I. M. Green has at last brought a startling clarity to the making of the Anglican Restoration religious settlement. Green, working at a number of levels, from parish to diocese to parliament and court, establishes that ejection of Puritan ministers was not at all immediate and initial reinstatements of sequestered clergy were not universally successful. In superbly ordered chapters the work reveals a strong willingness to accommodate on the parts both of Cromwellian incumbents and Anglican officialdom. King Charles is portrayed as at the heart of this spirit of accommodation, more than willing to merge the diversities of Puritanism with the establishment, most probably in order to secure a foothold for Catholic recusants. In the explicatory process, Green removes entirely Bosher's view that "Laudian" resurgence made the Act of Uniformity a *fait accompli*. For his own "tide of reaction" (p. 128) against the persistent Puritans, Green blames the gentry of the Cavalier Parliament, some of whom, interestingly enough, had earlier championed comprehension. It seems that, with the threat of inclusion for Cromwellian appointments, came the threat of loss of clerical endowments, of land control, and perhaps, though Green does not say so, of lay control of livings. To paraphrase James I, "no bishop, no propertied gentry." Thus, to the extent that the gentry felt threatened by any continuation of Cromwellian appointments—and the extent was great—they insisted upon ejection and uniformity.

Among other features worth comment is the discovery that the wealth amassed through recovery

finer came frequently from those incumbents who wished to pay only in order to assure tenure for livings that would have been adjudged legitimate in any case. Of dramatic quality is an assertion—though it will probably be challenged by other scholars—that “far more Commonwealth clergy conformed to the Restoration settlement, at least outwardly, than were ejected for nonconformity” (p. 177). Though this statement could mean a number of things, does Green really match, at least 2001 against 2000 ejections? Of course, what he could have relied upon, together with his superb evidence, is the knowledge that, especially in 1660, English Presbyterians were loath to encourage reputations for schism, that they openly expressed that they were opposed only to “prelatical” episcopacy, and that monarchy to them was certainly a human form of a divine institution. Thus Green could have said that the multitudes of Commonwealth clergy remaining in the Restoration church, at all levels, were willing to accept a *de facto* comprehension even at the risk of scruple. Sectarian lines still were not drawn, in many quarters, with the precision of insurmountable national borders as they are today.

This is a cogent exposition, and, though Green does not particularly relish the comparison he makes, it does place the Restoration settlement alongside the Elizabethan settlement of 1559. In both cases Erastianism made ejection a foregone conclusion, once Charles yielded to ejectionist pressures, but Green suggests that it was also a case of ruptured comprehension and that ejection certainly was not an obvious corollary to royal Restoration.

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MAURICE ASHLEY. *James II*. Minneapolis: University of Minnesota Press. 1978. Pp. 342. \$14.95.

Maurice Ashley justifies his decision to write a biography of James II by observing that there is no adequate modern account to counter the Whiggish prejudices of Macaulay and the rest. We can infer another motive from the prominence the author gives to the motto, “*Tout comprendre c’est tout pardonner*.” On the first count Ashley has set up something of a straw man; since the works of Jones, Kenyon, and Miller appeared, few people now believe that James hoped to impose Catholicism autocratically on his people. On the second count he certainly fails, for even if forgiveness is ours to grant we are left little nearer the goal of comprehension of that unwise and inflexible king.

This work shows every sign of having been written in great haste. In addition to biographers’ howlers (such as, “meanwhile what had been hap-

pening in England?”), careless errors and syntactical slips abound, culminating in the description on facing pages of James’s proclamation of September 21, 1688 as designed “to quieten the minds of his own people and to ensure their loyalty” in face of war with the Dutch, and yet “not intended to quieten the minds of people about the preparations for war known to be in progress in Holland” (pp. 236–37).

More damaging than the carelessness displayed is the apparent lack of historical understanding, surprising in one who has written so confidently on such a wide range of seventeenth-century subjects. When we read that the former Exclusionists in exile at the Hague were all “republicans” or that the Puritans under Elizabeth and James I supported freedom of thought for all Christians in order to gain it for themselves, our confidence in the author’s substantive conclusions is undermined. Perhaps Ashley’s main argument (in a work that is wholly narrative) is that James was completely and sincerely tolerationist (as, allegedly, was his brother Charles). This may or may not be true, but we can only bring in the verdict “not proven” on the case made here. As Ashley himself observes, Huguenot refugees after 1685 were expected to join the Church of England, which “was still largely Calvinist”; thus, he blithely asserts, they should have had no difficulty there. Though James allowed Scottish Presbyterians to worship in their own way, he would not permit field conventicles to be held because he regarded those attending them “as enemies of the monarchy”: most early modern persecuting monarchs would probably have made the same defense. On the reaction against toleration Ashley is equally unsatisfactory. He concludes that Cromwell’s institution of a form of toleration had been acceptable because he had provided for it in his constitutions, whereas Charles II and James II relied on their prerogative. “What James’s critics objected to was not so much the principle of liberty of conscience in itself but the use of the dispensing and suspending powers to attain his ends” (p. 199). One wonders why James was unable to achieve statutory repeal of the Tests.

Ashley shies away from issues on which he might have made a contribution. He tells us that James’s naval orders of 1662 omitted the “revolutionary” provisions of the 1653 Admiralty orders for line-ahead formation but fails to examine what this does for the one favorable item in James’s record, his career as admiral. He refers to a 1684 plan which would have effectively abolished representative assemblies in the New England colonies and which reportedly set Halifax at odds with James, but gives us no details. Perhaps worst of all, James’s Irish policy, both before and after 1688, largely remains shrouded in silence. One of the few

significant contributions is the description of the warm reception accorded James in Scotland during his Exclusion Crisis exile, when at last a Stuart prince was resident.

Readers wanting a detailed, if rather pedestrian, account of James's life may turn to this book. Those wanting the comprehension promised by the motto could still do much worse than to turn to the pages of Jones, Kenyon, or Miller.

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JOSEPH M. LEVINE. *Dr. Woodward's Shield: History, Science, and Satire in Augustan England*. Berkeley and Los Angeles: University of California Press. 1977. Pp. x, 362. \$19.75.

John Woodward (1667–1728), F.R.S., adherent of the “moderns” and of Baconian methods, remarkably learned and amazingly opinionated, was an important figure of the second rank in the intellectual life of his age. He is the central figure in this excellent study, which uses the disputes he was involved in as the focus for insightful discussions of assumptions, methods, and conclusions in the developing sciences of paleontology, geology, archeology, and history.

Woodward, for instance, was entangled in controversy about the nature of the fossils of ocean creatures found on hillside and mountain top. He held that the fossils were remnants of once-living animals deposited in high places by the great flood. He insisted that his conclusion was based solely on observation, a particularly happy resolution since it provided proof of divine revelation to Moses who managed to get the story right without having the information available to modern scholars. Joseph M. Levine dwells sympathetically on the plausibility of Woodward's views in the context of the science of his time, particularly noting those persuasive reasons that convinced nearly all students that the earth was only a few thousand years old. Yet, if interpretations were seriously flawed, these contentious virtuosi were creating the methods which would lead to more accurate conclusions. Levine finds similar situations in the related fields of geology, antiquities, and history. He is very good at redeeming the significance of faulty argument through showing the rationality in it and through demonstrating how in the midst of such debates a more rigorous sense of comparative empirical method was being established.

It is therefore a bit surprising to find him referring to the opinions of Woodward and his associates as “premature” (pp. 72, 83, and 103). If this is true in terms of the more accurate conclusions of later generations, there is another sense in which mistaken hypotheses were not so much premature

as necessary. The suggestion that general interpretation should wait until all the facts are in, implying the possibility of a purely empirical period of data gathering, ignores the extent to which empirical investigation itself is inspired and informed by general views of the subject. The hermeneutical circle operates at all levels of study, including the most elementary, a fact this work should help make clear.

There is much else in this book that makes it rewarding reading: enjoyable sketches of obscure figures such as Thomas Hearne and Henry Dodwell; interesting digressions into such matters as the personal rivalries of those who wanted to control the Royal Society; and lively accounts of the satirical literature aimed at pedantic virtuosi. Levine also has some discriminating things to say about the perspective dividing “ancient” and “modern” ways of viewing and using antiquity. This is an intelligent and instructive book, a model of how to make the study of a secondary figure rewarding.

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E. P. THOMPSON. *Whigs and Hunters: The Origin of the Black Act*. New York: Pantheon Books. 1978. Pp. 313. Cloth \$15.00, paper \$5.95.

At first glance, E. P. Thompson's first book since *The Making of the English Working Class* appears to be a new departure for him. He has gone back almost a century in time and has shifted his attention from the making of a social class to the making of one law, the Black Act of 1723. In fact, however, readers of Thompson will find themselves on familiar ground. He treats important themes that he has treated before—the erosion of customary rights and the conflict between folk justice and upper-class law—with the same combination of strengths and weaknesses—admirable research, a deep humanitarian concern, and a tendency to paint with too broad a brush—that we have come to expect from him.

The Black Act, which created at a stroke about fifty new capital offenses, has long been noted for its severity, but its origins have not been studied until recently. *Whigs and Hunters* is actually three studies in one. The first two examine the unrest and violence in Berkshire and Hampshire that the act was ostensibly meant to remedy. These sections show us Thompson at his best, and that, as we know, is very good indeed.

Working from what he admits is scanty and incomplete evidence, Thompson has re-created the complex life of two forest regions in southern England. The strict enforcement of the forest laws after the Hanoverian succession, he argues, was seen by

yeomen, customary tenants, and traditionalist gentry alike as an encroachment on their rights. They ultimately met this encroachment with violence, aimed not at economic gain, as at least one historian has claimed, but at exacting popular retribution.

The re-creation of these episodes has involved Thompson in an impressive exercise of historical detective work. He has had to eke out scanty sources ingeniously—at times, perhaps too ingeniously. He sometimes places too much reliance on some sources, accepting, for example, without reservation, the expense accounts submitted by the chief gamekeeper of Windsor Forest as evidence for the extensive activities of the poachers, even though it was to the keeper's advantage to exaggerate, a fact that Thompson himself notes when it suits his purpose (pp. 65, 220). He seems willing, as well, to accept most stories that have entered the folk tradition, even when he himself considers them incredible (p. 229, for example). For all this, however, there can be little doubt that Thompson has got the essentials of the story right. In so doing he has increased further our understanding of the moral economy of the eighteenth-century rural community.

It is the third section of the book, which attempts to account for why relatively minor and localized violence led to the passage of so harsh a law, that will be most controversial, and rightly so. Briefly put, Thompson believes that the Hanoverian Whigs, men who "had formed habits of mental distance and moral levity towards human life" (p. 197), passed the act in their own interest. He argues further than the Black Act reflects an important step in the transformation of England from a precapitalist to a capitalist society, with the Whigs cast in the role of the capitalists. There may be some truth to this model. The eighteenth century saw the rise of new men, many of them unlovely, and the mild reaction of the traditionalist gentry in Berkshire and Hampshire to the activities of the poachers attests to their own fear of an erosion of their old ascendancy. But this important perception is so buried under Thompson's sneers at "class avarice" and "capitalist nature" (pp. 221, 222) as to be almost lost.

Whigs and Hunters ends with a personal discourse on the importance of the idea of law in English society and its significance for the historian. These observations alone are almost worth the price of the book.

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JOHN CANNON, editor. *The Letters of Junius*. New York: Oxford University Press, 1978. Pp. xxxiii, 643. \$49.00.

The book jacket speaks of a demand for a "reliable edition" of Junius's letters, and in response here is Junius along with the poets in the distinguished Oxford English Texts series. John Cannon has satisfied the demand with the best edition of the letters, but it is unlikely that, in consequence, historical scholarship will take a great leap forward. Whatever excitement Junius's epistolary efforts added to public life from 1769 to 1772, they had only a "modest" (p. xxi) impact upon politics. His attacks may have weakened the Grafton ministry, but even without Junius it would have fallen, and the Duke's reputation would have been no greater. All Junius did, even if he helped bring down Grafton, was open the way for Lord North, not for the Grenville-Chatham-Rockingham coalition he desired. Happily for him, North and Lord Mansfield were alluring targets but were not vulnerable in one titillating way that Grafton had been.

The "by-products" (p. xxi) of his efforts, as they related to John Wilkes, freedom of the press, and, in the long run, to constitutional reform, may have been Junius's outstanding contributions, though again there may be differing judgments on the relative importance of his role. If Cannon had solved the mystery of Junius's identity, we might more precisely distinguish between personal malevolence and lofty patriotism as springs for Junius's actions. Cannon discusses this problem in an appendix. He makes a circumstantial case for Philip Francis as the "most probable contender" (p. 571). But because of the continuing uncertainty, a consideration of Junius's motives cannot be firmly grounded.

In any case, Junius expressed some noble sentiments about freedom in relation to the press cases of his time and the role of juries in libel cases. They deserve to be remembered because they remain pertinent. In the preface to the 1772 edition of the letters, which he himself prepared for publication, Junius argued for conceding to the press a wider latitude in discussing public than private men. Even if it is self-exculpatory, Junius's paragraph on the role of the press should be in every journalism textbook, for it gets to essentials.

Cannon is also modest in assessing the letters for their literary style and for their value as historical sources and discussions of constitutional issues. In all three respects, previous editors have claimed more for them. Cannon has done his job as editor very well. I commend him for being unobtrusive, for anticipating the needs and desires of a person who has to call upon Junius or who wants to know the political context of the letters, and for separating Junius's private letters from those written for *The Public Advertiser*. In the end it appears that, if we ever learn conclusively who Junius was, it will be as the result of an accidental discovery rather than meticulous historical detective work, so much

of which has already been devoted to the problem of his identity. Yet until it is known, Junius cannot be taken as seriously as he would have liked.

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H. T. DICKINSON. *Liberty and Property: Political Ideology in Eighteenth-Century Britain*. New York: Holmes and Meier. 1978. Pp. 369. \$35.00.

H. T. Dickinson has presented the central themes of *Liberty and Property* with great clarity. An examination of contemporary writings led him to conclude that Namierite preoccupation with power-brokers to the exclusion of the ideas that animated them has obscured the full picture of eighteenth-century English political life. From the 1680s to the 1790s Dickinson finds that Whigs and Tories, as well as court and country men, never ceased being concerned with questions about the nature of government. After the Glorious Revolution both Whigs and Tories were forced to reassess their intellectual commitments—the Tories, because continued insistence upon divine right monarchy and indefeasible hereditary succession would have plunged them into the civil disorders they so detested, and the Whigs, because many of the contentions hammered out in the Exclusion Crisis were ill-adapted to the exercise of power, particularly the monopoly of power they enjoyed after 1715. Because Tories persisted in emphasizing the need for absolute sovereignty, Dickinson argues, Whigs moderated their more radical pronouncements about government by consent. In turn the Tories slowly accommodated themselves to *de facto* monarchy. Since the Whigs shared Tory concerns with protecting property rights and prescribing social arrangements, they turned away from the abstract idea of a social contract among equals and stressed instead the historic contractual agreements represented in England's ancient constitution. Tories, too, found that they could best contribute to stability by supporting the historic political institutions of England. Thus veneration of the English constitution and exaltation of parliamentary sovereignty fused to form that conservative political piety peculiar to eighteenth-century England.

As party divisions became less relevant to the distribution of power after the Whig triumph, the functional differences between members of the government and back benchers made the court and country division more salient. Ironically the consensus achieved on the subject of the unique blessings of England's mixed constitution became the source of new tensions. With order, liberty, property rights, and the rule of law all riding on

the preservation of a delicate balance between king, Lords, and Commons, it is no wonder that everyone claimed to be preserving the constitutional balance. How this was best done became the issue. Court Whigs justified the use of patronage, because it secured the necessary working relationship between king and parliament; country critics saw the proliferation of placemen as evidence of the corruption that was undermining the independence upon which the constitutional balance depended.

While Dickinson has faithfully recorded what a good many different men said about the constitution, the right of resistance, and the origins of government, he has not faced up to the task of showing what bearing these assertions had upon the course of political events. Admittedly this is a very difficult thing to do, but without a closer association of content with context, political thought will remain in the limbo created by Sir Lewis Namier. One sometimes gets the impression that substitution of the adjective, ideological, for intellectual is supposed to do the trick. The concept of ideology does hold out the promise that the exploration of a particular conceptual order will reveal the parameters of political discourse and consequently of political action. But these are points to be made, not begged. In *Liberty and Property*, for instance, there is repeated reference to expressions of upper-class concerns about protecting property. No one, we are told many times, wanted a social revolution or believed that the laboring poor could be trusted with political rights. Since ruling groups commonly are attached to their privileges, why did the defense of privilege figure so largely in eighteenth-century writings? Why were uncontested points frequently dilated upon? Why, indeed, in the whole gamut of political propositions were some examined and other neglected? The word, ideology, suggests the presence of a sorting process going on in the common intellectual life of a society. Used as an analytical device, ideology should show how circumstances and social structures shaped this process at a given time and place; it should also indicate what beliefs were being held hostage for which patterns of behavior.

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JOHN POLLOCK. *Wilberforce*. New York: St. Martin's Press. 1978. Pp. xvi, 368. \$16.95.

Wilberforce has never lost his reputation as the key figure in the British abolition of the slave trade. John Pollock is his third serious twentieth-century biographer, but he has used a wider range of man-

uscript sources than any previous scholar. The result is a valuable study, though it is marred by unnecessary protests about Wilberforce's motives, consistency, and concern for the domestic poor.

Like Wilberforce, Pollock is interested in much more than slavery and the slave trade. One of his organizing themes is the change in British values before the Victorian period. Wilberforce's general aim was "to turn 'eighteenth century' gentlemen into 'nineteenth century' gentlemen" (p. 64), with an ethic of social responsibility which accepted the corrosive effects of immorality and recognized that "in pauperism as in slavery, the degradation of character deprives the individual of half his value" (p. 141). Within this wider context, he shows a Wilberforce soldiering tirelessly for forerunners of Victorian reform like the Bettering Society and the Bible Society and working in campaigns like Hannah More's attempt to educate the peasantry in the Mendips, Tom Erskine's attack on bull-baiting, Elizabeth Fry's efforts at prison reform, and Samuel Romilly's agitation for penal reform. After the triumph of 1807, too, Pollock presents him as a political figure not formally aligned with either party, but capable of carrying uncommitted members with him to what he saw as the moral side of any issue under debate.

Pollock himself is bewitched by Wilberforce, and at times his vignettes are delightful. Wilberforce, the lover of plants, people, and animals, entranced by a moss rose, enthusiastic about a pet hare, or besieged by supplicants at his extraordinary charitable levees in Gore House is almost Dickensian in his eccentricities, a much more congenial figure than the prosecuting moralist of the Proclamation Society. Pollock draws no distinction between private kindness and public behavior. He is convinced that both, including the attack on the slave trade, were part of the same pattern of widening benevolence based on an intensified conception of religious duty.

Pollock's interpretation is both too defensive and insufficiently critical. He protests vigorously against contemporaries like Cobbett or historians like the Hammonds, whose assumption that the evangelicals were hypocritically indifferent to British poverty is too crude to be dignified by argument. At the same time, Pollock is unwilling to recognize feet of clay. In the election of 1807, for instance, it is still hard to believe that Wilberforce did not know his Tory supporters were using his influence to help the slaveholder Beau Lascelles against Lord Milton. This is less important than Pollock's failure to explore the anxieties which lay behind the commitment to evangelical Christianity. Religious declension and immorality have been common to most ruling classes, and Pollock does not try to explain the change in values that he

illustrates through Wilberforce's career. Moreover, he does not consider the possibility that slavery became an urgent matter to evangelicals precisely because of its symbolic force in soothing anxieties over alarming domestic conditions. This approach need not exhumate the old red herring of conscious hypocrisy or deny the social force of religious motivation.

This is still a most useful biography. It is based on prodigious manuscript research. It is deeply sensitive to Wilberforce and, in particular, to the way in which his behavior was influenced by his changing medical condition and his use of opium. Pollock makes a real contribution to the history of reform in the Atlantic world. His Wilberforce is the archetype of the nineteenth-century reformer, confident that social abuses could be eradicated, not by changing society but by promoting its awareness of Christian responsibility.

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FOLARIN SHYLLON. *Black People in Britain, 1555-1833*. New York: Oxford University Press; for Institute of Race Relations, London. 1977. Pp. x, 290. \$19.50.

Although the title may suggest a social history of persons of African descent in Britain, the author lacks sufficient information to suggest any startling generalizations or to make quantitative analyses. Instead Folarin Shyllon relates the black experience by bringing together separate items of information about individuals who were slaves, servants, students, laborers, artificers, visitors, and beggars. The last third of the book comprises brief sketches of the lives of eight Africans who lived in the late eighteenth and early nineteenth centuries. Included are Ottobah Cugoana, Olaudah Equiano, and Ignatius Sancho who are best known for their writings, Ira Aldridge the actor, and George Bridgetower the musician. In part two, entitled "Black Poor," the author concentrates on the founding of Sierra Leone in 1787. He concludes that "the Government and the liberal and reactionary Establishment" cooperated "to preserve the purity of the English bloodstream by expelling from England the 'lesser breeds without the law'" (p. 128). This interpretation of the motives of Jonas Hanway, and of Henry Thornton, William Wilberforce, and their associates in the Sierra Leone Company seems to this reviewer a complete misreading of their aims. Granville Sharp is one of the few white abolitionists who continues to be viewed as a true friend of the Africans. That such a small proportion of blacks could be persuaded to go to Sierra Leone is seen as a victory of black solidarity over British racism.

There is little information concerning Africans in Britain in the sixteenth and seventeenth centuries. By far the author devotes greatest attention to the last third of the eighteenth century. Shyllon believes that there were about ten thousand blacks in Britain at that time, though why he prefers that estimate over Granville Sharp's estimate of double that number is not persuasive. What is clear is that it was a tiny percentage of the total population, concentrated in London, with lesser numbers in Liverpool and Bristol. Blacks were most often employed as domestic servants. Others worked as laborers and tradesmen. Some were self-employed. So many were forced to beg for a living that they drew attention as a nuisance. Persons of African descent felt a community of interest and in London met together from time to time to celebrate Mansfield's Somerset decision, to petition against the slave trade, and to enjoy themselves with music and dancing.

The author is concerned not only with the historical experiences and achievements of blacks but also in showing "that racism has been the British way of life ever since the first blacks settled in Britain" (p. x). A moralizing tone, now of praise and now of condemnation, is characteristic of much of the book. At times there seems less a desire to explain than a compulsion to accuse.

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ANTHONY J. BARKER. *The African Link: British Attitudes to the Negro in the Era of the Atlantic Slave Trade, 1550-1807*. Totowa, N.J.: Frank Cass. 1978. Pp. xi, 263. \$22.50.

In *The African Link* Anthony J. Barker carries the study of English racial attitudes back to the earliest English contact with Africa in the sixteenth century. Thus his book serves as a foundation to Philip Curtin's *Image of Africa*. Unlike most assessments of racial prejudice, *The African Link* focuses not on accounts of the Africans as slaves in the Americas and the Caribbean but rather on descriptions of Africans in Africa and the few Africans resident in England.

Barker contests the view that the racial attitudes accompanying the abolition movement of the late eighteenth century were newly developed and represented an "assault on entrenched ignorance and indifference" about Africans (p. ix). On the contrary, he establishes that by the end of the eighteenth century a great deal already was known about the continent and its inhabitants, and he demonstrates that the intellectual fodder used by both sides in the abolition controversy derived from a solid body of familiar descriptive and ana-

lytical material that had appeared over the course of two centuries. He also maintains that Edward Long's relegation of Africans to a position outside and below the human species was not, as scholars frequently have contended, a natural development out of eighteenth-century thought and information about Africans but rather an aberration tangential to existing philosophic constructs. That is not to say, however, that Barker finds the ethnocentric English to be without prejudice. His thorough investigation of first-hand accounts, popular encyclopedias, histories, and geographies uncovers the existence of a widely accepted and disparaging African stereotype. Africans were considered to possess a lower culture and to be ugly, savage, heathen, sexually promiscuous, and incompetent. Yet the commentators held that African inferiority was not innate but circumstantial, and they attributed it to such conditions as environment and a lapse into a state of degeneracy. It was not until Long gave wide currency to Samuel Eastwick's contention that Africans were a distinct and lower species that their humanity and human potential were denied. Hence, the emphasis on the humanity of the Africans during the abolition debate was a restatement of an accepted view rather than the assertion of a new one.

On the whole Barker argues his thesis convincingly. He investigates the appropriate avenues, be they theological, geographical, or philosophical. He is aware of the limitations of his sources and he never claims too much. His arguments progress clearly and are well supported, and he challenges persuasively the theses of several scholars who have gone before him. Although additional work undoubtedly will be done on aspects of the subject, Barker's book will stand for some time to come.

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MORRIS BERMAN. *Social Change and Scientific Organization: The Royal Institution, 1799-1844*. Ithaca, N.Y.: Cornell University Press. 1978. Pp. xxv, 224. \$17.50.

Morris Berman's book is not merely a history of the first half century of the Royal Institution's existence. It is that, but it is very much more besides. He tells us that his work is "an attempt to use England's first major scientific laboratory to ask what I hope are significant questions regarding the nature and function of science in industrial society." And he explains that he has not "tried to 'recapture' the Royal Institution of 1799-1844 as much as I have tried to clarify its role in the creation of modern scientific society, and the objective conditions that made that role possible."

He succeeds in this attempt and at the same time demonstrates how a genuinely scholarly historian can profit from the insights and methods of sociology. Berman is not concerned to probe the problem of the validity of scientific knowledge and its unquestioned primacy in the value system of contemporary Western man. Rather, he sets out to try to understand "the historical process by which science became organized so as to serve, almost literally, as the grammar of industrial society: its set of rules, its ideology." His analysis is, therefore, not epistemological, but sociological; and for this he uses careful historical research.

When the Royal Institution was founded in 1799, the Royal Society was already nearly a century and a half old. But the Royal Society had long since ceased to be a vehicle for the organization of science in England and had become instead a club fostering the "gentleman amateur tradition." By the end of the eighteenth century there was a need in England for an establishment like the Royal Institution, a need felt by those concerned to meet the pressing social and political problems of the day with "scientific" solutions. During the first decade of the RI's existence, its promoters were largely agriculturalists, large landowners dedicated to improving their agricultural situation through experimentation with new crops and machinery and by opening up mines, canals, or turnpikes on their property. These were by no means the majority of England's great landowners, but they were articulate and visible, and they used the scientists and scientific apparatuses of the RI for their purposes. Shortly after 1811 the agricultural interests at the RI gave way to the interests of industrialists (many of whom were Benthamite Utilitarians), so that owners of coal mines and gas works and tanneries, together with their middle-class peers in government, came to rely on the RI both to help solve their industrial problems and to give "scientific" sanction to their political solutions.

Berman uses the careers of the RI's leading scientists to demonstrate his theme and to show how these men and their work at the RI came to mold public attitudes about science and its relationship to industrial society. His accounts of Sir Humphrey Davy and Michael Faraday are especially well drawn, and they show both men in lights and with nuances not previously expressed. Toward the end of his book Berman tells the story of the famous explosion in September 1844 at the Haswell Colliery in the Durham coal fields, which caused the death of ninety-five men and boys. There was a government inquest, and Faraday, among others, was asked to testify. The mine owners were eventually exonerated of any culpability, and this was primarily due to the "scientific" testimony of men

like Faraday, even though the testimony had almost nothing to do with the actual situation at the Haswell Colliery. "Here was a mine disaster in an area recently torn apart by a strike," Berman concludes, "a disaster that could have triggered off another strike or even a riot. Instead it had been gently subdued by a technological fix that was actually inadequate. The legal ideology of science was ultimately the ideology of containment."

What is at issue here is the question of real personal and social priorities. Industrial society (for which, read "those who profited greatly from industrial enterprises") needed, and still needs, "scientific expertise" to justify it to the public. Through the Royal Institution and its scientists in the early nineteenth century, the British public came to believe that industrial society as interpreted by the scientific experts was not only good but necessary. Berman's book queries this assumption and shows that the perception of reality in scientific terms is not necessarily the perception of reality and certainly never the perception of the whole of reality. The Royal Institution, more than any other structure in British society, helped to reconcile the masses to the scientists' perception of reality. "The question facing Western society today, or so I would think," Berman writes, "is whether we wish to be so reconciled any longer, and whether the dominant culture can ever break free from this not-so-objective definition of scientific objectivity."

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G. N. VON TUNZELMANN. *Steam Power and British Industrialization to 1860*. New York: Oxford University Press, 1978. Pp. x, 344. \$31.00.

G. N. von Tunzelmann's important study of the impact of the steam engine on the factory textile industry should be read by everyone with serious interest in the Industrial Revolution, the history of technological change, or the new economic history. This is economic history at its best; the combination of the logic of economics and the diligence of historical research result in a revision of our view of an important issue. The steam engine was less important than we thought.

Careful study of available evidence—much of it archival (the bibliography would have been strengthened by a bit more detail)—indicates that Watt's steam engine and the steam engine in general had only modest impact on British national income. If all of Watt's engines were replaced in 1800 by Newcomen engines—pumping water to turn water wheels where rotative power was needed—the extra cost would have been about

£200,000 or about 0.1 percent of national income. The actual cost of Newcomen engines, which can be determined with some confidence, almost certainly exceeded the cost of foregoing Watt's invention since, in many cases, water power would have been cheaper than a Newcomen engine. The cost of doing without steam power entirely is a more uncertain calculation. Information is meager, and the problem of estimating rising costs and site rents for water power is given expert and lengthy consideration. The best guess of the net social cost of going without steam engines is, somewhat surprisingly, in the same order of magnitude as the cost of replacing Watt engines with Newcomen engines. The extra cost to power users would have been £1.4 and £1.8 million, but all except £165,000 to £330,000 would have gone in rents to owners of water rights and would thus not represent a real income loss. Interestingly, the greatest savings from Watt's engine occurred in Cornwall's great pumping engines (about a third of the total) while the savings in the industrial regions of the Midlands and the North were modest.

The second half of the book evaluates the impact of the steam engine on the textile industries. This is a major contribution to the history of these industries and involves the creation of several revised data series for inputs and outputs. The diffusion of power technology is explained within a context of cost-minimizing behavior. Steam power appears to have had its greatest impact when high pressure engines were introduced in the 1940s, not during the Industrial Revolution.

The book is occasionally marred by methodological self-consciousness. There are a few unnecessary methodological comments, but von Tunzelmann appears to desire to keep his analysis unobtrusive. More than once the reader would have been helped if data behind a quantitative conclusion had been collected from the preceding dozen pages and presented in a table. More serious is the failure to explain the difference between the extra power cost to users if water had replaced steam and the social cost. This is a consequence of the need to use increasingly expensive water sources and is relatively easy for an economist to follow, but it will be unintelligible to those without economic training. It could have been explained in a few hundred words.

Von Tunzelmann is both careful and skeptical in his calculations; he emphasizes they can only indicate orders of magnitude. The reader, nonetheless, is convinced that Watt's engine did transform an economy caught in an "energy crisis." Rather this study of steampower and the factory textile industry adds to growing evidence that modern economic growth consists of pervasive

technological changes, each with modest impact, rather than revolutions and take-offs.

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DAVID W. HOWELL. *Land and People in Nineteenth-Century Wales*. (Studies in Economic History.) Boston: Routledge and Kegan Paul. 1978. Pp. xiv, 207. \$15.50.

This is an important and challenging work. It is first and foremost a history of agriculture in Wales during the nineteenth century. As such it fills a great gap in our knowledge while at the same time redirecting academic attention toward what always tends to be taken for granted, namely, those deep rhythms in the immemorial life of the countryside that, despite the breakneck speed and revolutionary changes of industrialization and urbanization, nevertheless continued to constitute the source of much that was distinctive in Welsh culture throughout those momentous years.

The author is primarily an economic historian, and he is at his best when dealing with purely economic factors or those aspects of his subject which are amenable to economic analysis. Thus, his chapter on the pattern of land-ownership and the distribution of estates of varying sizes is excellent. So also the chapter on occupancy and the size of holdings, and on marketing and farming practices. The main characteristic of Welsh agriculture was its backwardness, which he attributes mainly to the extreme conservatism of the peasantry and the debilitating effects of a lack of investment capital or, in the case of the lesser estates, of a failure to invest. The large estates come out well in his reckoning. Backwardness, of course, is a relative term, and too often one is left wondering whether the constant references to "English" agricultural practices might not be misleading and whether the comparisons should not more properly be made between similar farming regions. Nevertheless, this part of the work is worth careful study, based as it is on a wide range of sources; good use was made not only of the most important official papers but also of a wide range of manuscript sources.

But the book aspires also to make a major revision in Welsh social and political history. Here, it must be confessed, David W. Howell is a less certain guide. It seems to this reviewer that his work is based on a model that is defective and on evidence that is sometimes partial and insufficient in variety and scope. His theory is that the social structure of the "typical" Welsh farming community was a function of its economic relations and

determined not by kinship and culture but by geographical isolation, residence, backwardness, and relative poverty. This model leads Howell, logically enough, to conclude that the non-conformist radical politics so characteristic of Wales in the last century was the creation of a band of ruthless politicians, a kind of conspiracy devoted to the destruction of the ancient deference systems of the estates. Most of the author's evidence for this consists of the obiter dicta of the estate agents or statements culled from official papers. Much evidence of a different kind, particularly in the Welsh language, in the form of articles, poetry, and other writings and speeches is ignored, and, in any case, generalizations about the nature of politics can usefully be made only on the basis of studies of political situations—and these have their own distinctive methodologies. Nevertheless, despite these caveats, what Howell has to say about politics is worthy of careful consideration, and the substantive part of his book on the agricultural economy itself is definitive and of permanent value.

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DAVID V. ERDMAN, editor. *The Collected Works of Samuel Taylor Coleridge*. Part 3, *Essays on His Times*, in *The Morning Post and The Courier*. In three volumes. Princeton: Princeton University Press. 1978. Pp. clxxix, 436; x, 489; x, 526. \$75.00 the set.

This is the sixth part to appear in a projected sixteen part collection of Coleridge's works issued by Princeton University Press under the sponsorship of the Bollingen Foundation. Only such sponsorship could enable the publishing of a work today comparable in scope and enterprise to Julian P. Boyd's *The Papers of Thomas Jefferson*. Comparable also in meticulous editing, the texts so far have featured learned introductions, judicious notes, and full indexes clearly showing that these volumes were designed as lasting works of reference to Coleridge the man and to his times.

The present part, made up of three volumes, comprises journalism that Coleridge produced during twenty years (1798–1818) for the London *Morning Post* (vol. 1) and the evening *Courier* (vol. 2), with one entire volume given over to five appendices, chiefly of "conjectural or collaborative attributions." The large number of these conjectural essays marks the magnitude of David V. Erdman's accomplishment in identifying the more than two hundred essays he prints in the first two volumes.

A prefatory note explains the general principles

behind these identifications, and each essay carries a note explaining the grounds of its attribution. Many of the essays had been included in an earlier collection, by Coleridge's daughter, with virtually the same title (3 vols., 1850), but Erdman started anew some twenty-five years ago, justifying each attribution with external evidence of varying reliability while relying on distinctive internal evidence for certitude. He now provides a cautiously exhaustive collection of almost seventy essays in volume one and double that number in volume two, all carefully justified as Coleridge's own.

Erdman's lengthy introduction is a model of its kind, an overview followed by a detailed narrative of Coleridge's principles and practice, in a style both erudite and light, as in calling Bonaparte "as safe an object of abuse as sin" (1: cxl). Both here and in the copious notes, the style leavens what could easily have become tedious because of the need to compress a great deal of information about a complicated career.

That career reflects three distinct phases of industrious journalism, 1797–98, 1799–1804, and 1811–12. In the first, we see the Coleridge that most schoolchildren know, the radical who fulminated against the French War, "an evil thing" weakening the entire fabric of the nation. Erdman makes clear the nature of that early radicalism as less populist than moral, with Coleridge assuming the role of "state physician" ministering to the "guilt of his nation and of all mankind" (1: lxi). Thus his subsequent support of the war against France when Napoleon appeared is made to seem perfectly consistent. For Coleridge saw as evil Napoleon's character as well as conduct, a thesis most fully developed in the Plutarchian sketch, "Pitt and Bonaparte" (1: 219–26), of which he boasted, "Since the Time of Junius no single Essay ever made more noise in a newspaper than this" (1: 226n).

Coleridge was no Junius. Erdman shows him oscillating but consistent: "He is in truth 'ever the same' in the sense that he is never single-sided or single-minded but always both Jacobin and anti-Jacobin, Radical and Tory, poet and moralist, intermingled" (1: lxv). His concerns, like those of Junius, were with current issues rather than long-range goals, but the political principle behind the essays seems the opportunism of a political enthusiast more than the expediency of a political thinker.

This judgment does not devalue the essays as political documents if, as I believe, they were meant to be "political" in the ancient sense that conceived of the nation as a framework for fostering the whole activity—spiritual, moral, intellectual—of man and all mankind. In this view,

the essays, even when mere hackwork, are to be read less for what they tell us about Coleridge's political thought than as lively, evocative responses to current events and the men who made them.

Because they focus so tightly on details of ministerial policy and party infighting, the essays require extensive notes. Erdman's comments, erudite but unobtrusive, help us to understand even Coleridge's habitual punning—as, in commenting on the bullion controversy of 1811, when he calls the debaters, “Crow King, Jo. King, No. King, and Thynne King” (2: 251). And for our further pleasure, we have cartoons by contemporary masters like Gillray and Cruikshank. But surely the best sign of the thoroughness behind this book is the index that covers two hundred pages.

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G. I. T. MACHIN. *Politics and the Churches in Great Britain, 1832 to 1868*. New York: Oxford University Press. 1977. Pp. viii, 438. \$34.50.

G. I. T. Machin's comprehensive survey covers an era that witnessed “the extension of religious plurality in a non-authoritarian society” (p. 380) and the elaboration of church-state issues that frequently dominated political life. There is no other work that treats this important subject in such breadth and detail; the inclusion and integration of both Scottish and Irish issues is a remarkable feature of the book. Based on extensive manuscript documentation as well as wide secondary reading, it is nonetheless a work of synthesis and analysis rather than an original contribution to knowledge. To this, however, there are two exceptions, both anticipated by earlier Machin articles: the best explanation yet of Russell's Durham Letter (“a pondered missive from the leader of a Liberal Government and not the hasty screed of a compulsive anti-Papist” [p. 210]) and a careful study of the development, not inconsistent with his earlier Tractarianism, of Gladstone's growing rapport with Dissenters. The main part of the book is a close study of the counterpoint of religious pressure groups to parliamentary politics. Machin, essentially a political historian, is less interested in the religious dimension of religious history (except for Tractarianism) than in the role of religion as a source of political problems. The narrative is largely organized by ministries and parliamentary sessions, proceeding year by year, blow by blow, succeeding in integrating the various national, religious, and political strands of a complex story but somewhat restrained in making synthetic generalizations.

Machin sees the religiopolitical problems of this era, in which the assumption of the unity of church and state had to be abandoned, as twofold: the struggle for full liberty by nonestablished denominations and the issue of control within the Anglican and Scottish Establishments. Governments, both Whig and Tory, desired to retain Establishments and maintain their Erastian character while making measured concessions to Dissenters. Whig concessions, particularly on church rates, were too little and too late to prevent the rise of Voluntaryism among radical Dissenters (by no means all Dissenters); Machin provides an excellent study of the early history of the Anti-State Church (later Liberation) Society. But the warfare of Dissenter versus Churchman (or the parallel struggle of Irish Catholic versus Protestant) is only one-half of Machin's story. Another part concerns the reaction against Erastianism within the Establishments, movements seeking greater self-government by the church: Tractarianism within the Church of England and (an unlikely but remarkable parallel) the Non-Intrusionist Seceders among the Scottish Calvinists. Yet another cross-current is supplied by the ultra-Protestant reaction against both Tractarianism and Roman Catholicism. Many of these tendencies canceled each other out politically; despite the importance of religious issues in elections such as 1847, 1852, and 1857, the result was likely to be governments beset by such mutually incompatible religious pressures as to be unable to act effectively. But the long-term tendency to liberal concession was irresistible. Machin ends his story at the point when church rates had been abolished, Irish disestablishment was imminent, and British disestablishment was a vivid possibility. He ends with a question mark, and somewhat abruptly; one wishes he would carry the narrative, dealing with forces already in motion, at least as far as 1874. But the denouement, in which disinterest replaced disestablishment, would be another book.

JOSEF L. ALTHOLZ
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Twin Cities

ANTHONY S. WOHL. *The Eternal Slum: Housing and Social Policy in Victorian London*. (Studies in Urban History, number 5.) Montreal: McGill-Queen's University Press. 1977. Pp. xxiv, 386. \$29.95.

City slums remain one of the most powerful symbols of the dark side of modern urban life; efforts to understand and to change them therefore pose a central problem for the urban historian seeking to interpret the responses of city dwellers to their environment. Anthony S. Wohl, in his impressive,

aptly titled book, *The Eternal Slum*, studies changes in housing policy in London between 1840 and 1914, as he traces the evolving social theories and attitudes of the Victorian middle class toward government interference in the free market. The cast of characters in Wohl's book is large: we see doctors, philanthropists, parliamentarians, and, finally, local pressure groups contending in the arena of public policy, all battling the social evil of the slum and all being vanquished by its intractability. Wohl shows how an evolving series of coalitions and campaigns changed responses to London's substandard housing from local recommendations for drains, whitewash, and water pipes to the growing willingness to accept municipal responsibility for rebuilding and maintaining workers' apartments. He charts with precision a slow but steady drift away from *laissez faire* toward governmental intervention as the housing question forced administrators to recognize the social and economic roots of overcrowding, along with the inability of either charity or the marketplace to ameliorate conditions.

Wohl chooses his subject and his terrain well. Although the nineteenth-century French model of urban reconstruction has been extensively analyzed in studies of Baron Haussmann, the English model of piecemeal changes pushed by local authorities, medical officers of health, M.P.'s, and party leaders has remained much more obscure, in large part because of its complexity and its lower levels of accomplishment over short periods of time. Wohl has skillfully dissected, however, the divisions of responsibility and the diverse sources of energy for change and has demonstrated a sensitive understanding of urban social policy.

At the center of the book stand investigations of three linked topics: the public's changing perceptions of workers' housing, the efforts of groups such as the Peabody Trust and the Charity Organization Society to rebuild and renovate, and the actions of local and national governments. Wohl deftly interweaves these concerns, resurrecting both the personalities and the accomplishments of the major actors in his story. His skeptical, but not unsympathetic, treatment of Octavia Hill and other reformers highlights both the strengths and weaknesses of delegating urban housing policy to middle-class philanthropists. He meticulously traces what was built as well as what was left undone, all the while commenting upon the broader implications of his subject. The book is informed throughout by high scholarly standards and a concern for the social inequalities being tolerated and perpetuated by many of the people described.

Ending the study in 1914 is anticlimactic, however. Since Wohl's subject on one level is the slow

growth of the welfare state, the reader deserves some attention paid to later planning acts and the massive shift of social resources into council housing after World War II. The shadows of these changes loom in the background without being acknowledged. The other missing character is that of the slum's tenantry. Throughout the book, London workers are overcrowded, dispossessed, rehoused, shifted to the suburbs, and described and pitied by middle-class observers, but they are given no chance to comment on their environment. And slums seen in another light are workers' neighborhoods with their own integrity and cultural life. Surely another perception of the housing problem to be investigated is that of the slum dweller.

LYNN HOLLEN LEES
University of Pennsylvania

F. H. W. SHEPPARD *et al.* *The Grosvenor Estate in Mayfair. Part 1, General History.* (Survey of London, volume 39.) London: Athlone Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1977. Pp. xvi, 236, and 56 plates. \$40.00.

To the student of urban estate management and the dynamics of urban growth, this is the most important volume of the Survey of London yet to appear. Until recently the archives of the Grosvenor estate—the largest, wealthiest, and most successful of London's leasehold domains—were unavailable to the scholar. Hermione Hobhouse was the first to make use of them in her biography of Thomas Cubitt, but here their riches are exploited to the fullest. Volume thirty-nine deals only with the "Mayfair" estate—which extended from the north side of Berkeley Square to Oxford Street and from Park Lane to the boundary of the City of London's Conduit Mead estate—not with the Grosvenor properties in Belgravia and Pimlico. Detailed descriptions of individual buildings will follow in a second volume.

The new evidence shows once again the impossibility of generalizing about the operation of the London leasehold system, which varied not only from estate to estate, but on the same estate from period to period. While the property was first being developed—rapidly in the 1720s and 1730s and more slowly during the succeeding four decades—the ground landlords and their agents exercised minimal supervision over builders and tenants. Building agreements and leases were far less stringent than on the Bedford estate. There were no architectural controls. Even noxious trades were not prohibited but only discouraged by the imposition of an extra rent. Throughout the eighteenth century, "much seems to have been left to

the good sense of builders and future tenants" (p. 15).

A similar policy of noninterference, pursued in the early nineteenth century by the Northampton estate in Clerkenwell and Islington, produced noisome slums; but the inherent desirability of the Grosvenor property enabled it to become and to remain the resort of wealth and fashion. Three dukes, six earls, and a viscount lived in Grosvenor Square in 1790. The authors conclude that "favourable topographical situation and the absence of adverse social influences from surrounding areas were of more importance in preserving the original social quality of an estate than either the terms of land tenure at the time of first building or the watchful management of a ground landlord intent on maintaining the value of his property" (p. 98).

Not until the accession of the second Marquess of Westminster in 1845 did the estate begin fully to make use of its powers to restrict the activities of its tenants and to rebuild and reshape the property as leases fell in. By the latter years of the century, the first Duke of Westminster was able to impose and enforce schemes of comprehensive urban renewal with a degree of success his fellow landlords could only envy. Individual details of the district as it stands today—the pillared portico of a house in Upper Brook Street, the terracotta facing of a block of flats in Mount Street—reflect the particular tastes and express commands of the second marquess and the first duke.

The *Grosvenor Estate Strategy for Mayfair and Belgrave*, published in 1971, shows both the continuing vitality of an older tradition of private town planning and its applicability to present circumstances. More concretely, Grosvenor Square and the surrounding streets serve as models of urban virtue, monuments to the good sense of the original developers and to the intelligence, farsightedness, and diligence with which the Grosvenor Board took advantage of its powers to make the estate at once lucrative to its owners and lessees and a source of delight to the rest of us.

DONALD J. OLSEN
Vassar College

REBA N. SOFFER. *Ethics and Society in England: The Revolution in the Social Sciences, 1870–1914*. Berkeley and Los Angeles: University of California Press. 1978. Pp. 325. \$16.50.

Readers of H. Stuart Hughes's *Consciousness and Society* have often noted the conspicuous absence of English social and political thinkers from his useful survey. One radical pathologist of English "cultural sclerosis," Perry Anderson, has even raised this into an indictment. Where were the English

Freud, Weber, Durkheim, and Pareto? What was going on in England while the Continent was producing such intellectual revolutionaries? Anderson claims that England was sunk in a complacent, parochial torpor, but Reba N. Soffer argues otherwise. Indeed, she suggests that England too produced a "revolution in the social sciences," a Kuhnian revolution of paradigms. Yet having read her book, one is left bemused by so discreet a revolution and wondering if perhaps a more appropriate, if less dramatic, subtitle would have been "a study in continuity." Alfred Marshall, William James (an honorary Englishman), and Graham Wallas are designated here as revolutionaries in their respective disciplines, yet they emerge vividly from Soffer's evidence as late exemplars of that magnificent Victorian species—the energetic, individualist gentleman-altruist, the peculiarly unalienated sort of English intellectual that Weber so admired and whose rarity on the Continent made Weber's own work, on the sociology of bureaucracy, for instance, so compelling.

These revolutionaries are the heroes of Soffer's study. Together they sought an integrated, humanistic social science that would recognize the centrality of ethical considerations while achieving the methodological rigor necessary to professionalism. They are given further unity in this study by a generational approach that seems reasonable in general but shows certain rough spots, such as a reference on one page to "the generation of the 1880s" followed closely by a reference to "the generation of the 1870s" that seems to refer to essentially the same group. Born into the mid-Victorian calm, educated at the reformed universities, they matured in the troubled later decades of the century in which the confident social prescriptions of the 1850s and 1860s no longer seemed applicable, hence their revolt against an "established tradition of thought."

In developing this argument Soffer probably overstates the intellectual watershed; she posits a coherent, rationalistic orthodoxy in Victorian social theory, particularly economics and psychology, which may be appropriate as a textbook generalization but largely ignores variations and divergences, particularly those that suggest links with the ideas of her revolutionaries. The doctrines of Comte, for instance, skillfully propagated and naturalized by his English disciples, anticipated some central "revolutionary" tenets, such as the necessary link between ethical considerations and social science, the fallacy of excessive intellectualism in explaining human behavior, and even the tentativeness of knowledge. Not by coincidence was Wallas deeply interested in Comte's doctrines. It is nonetheless true, as Soffer remarks, that Comte's ideal of an indivisible social science

was not conducive to disciplinary specialization and professionalization.

The book has its heroes as well as its villains (perhaps the term is too strong), and, as is often the case, the villains seem rather more interesting. Soffer does not conceal her disapproval of the social psychologists, led by Francis Galton and Karl Pearson, who are stigmatized as "revisionists" because they did not break cleanly with traditional nineteenth-century thought but rather sought "to salvage its heart by transplanting it into a more robust body of theory" (p. 3). This led to the study of eugenics, the genetic determinism of which led, in turn, to elitist and illiberal social and political doctrines. Yet one might as plausibly argue that these were the revolutionaries, since their ideas carried them further than Marshall, James, and Wallas from the Victorian liberal intellectual consensus.

In short, this book raises important questions and offers a provocative synthesis, which, though it can be disputed at various points, is of considerable interest.

CHRISTOPHER KENT
University of Saskatchewan

G. M. YOUNG. *Portrait of an Age: Victorian England*. Annotated edition by GEORGE KITSON CLARK. New York: Oxford University Press. 1977. Pp. 423. \$38.00.

In 1934 G. M. Young edited a two-volume work, *Early Victorian England 1830-1865*, and wrote for it an eighty-nine page concluding essay that summed up the period in a manner at once brilliant, elusive, epigrammatic, and informed. In 1936 he extended that essay in an increasingly elliptical way up to 1901. Within a short compass and using a wealth of material from contemporary historical and literary sources, Young traced the triumph of the critical cast of mind of utilitarianism and evangelicalism to its apogee in the 1850s, and then the decline of that achievement. But this is hardly to suggest the richness of detail contained in so brief a space: the topics illuminated range from the role of Benthamism to the significance of the Boer War.

In the separate edition Young included a fascinating chronological table, year by year, from 1830 to 1902, of figures who were at the peak of their powers in a particular year, deaths, publications, art and architecture, public affairs, and other events. And both in the original and the second publication there were accompanying "illustrations"—reproductions of columns from the *Times*. These "illustrations," as well as the chronology, have been left out of the edition under review and one regrets their absence.

One is profoundly grateful, however, for what has been added. For in this edition there are 206 pages of annotations to the text, the vast majority of them written by George Kitson Clark. There is an exhilarating appropriateness about the entire endeavor. One can only regret that the book was not published before Kitson Clark's death in December 1975. (Young died in 1959.) For in this present work two past masters of Victorian England meet on the printed page. They have come together for a dialogue that is essential reading for anyone interested in nineteenth-century England. Here are present mostly complementary but at times conflicting approaches to the English past, written by men who have done so much to shape how we see the Victorian period.

Portrait of an Age has been available for some time in an Oxford paperback (and one hopes that this edition will be as well)—it is a masterly essay which should be well thumbed by the beginner (who will be stimulated and confused) and by all students of the period until they abandon their studies. Young's essay reveals both the strengths and weaknesses of the English "amateur" tradition—an extraordinary range of knowledge, which from time to time collapses into knowingness. Almost any sentence in the book is intriguing, and whether one agrees with it or not it could provide the text—and has, I suspect, frequently done so—for a thesis or a monograph. In that sense, too, the work repays rereadings, for sentences passed over on one reading have a way of standing out on the next.

If *Portrait of an Age* is a continuing and rewarding challenge to how one thinks about the nineteenth century, it is also a tease—to see how many of the references, the quotations, mostly in English but a few in Greek and Latin, the reader can recognize. It is this task of recognition and elucidation that Kitson Clark has set himself. It began as a game, then as a lure to bring new readers to what he regarded as a seminal essay. Kitson Clark was in the next generation of historians of Victorian England, more professional and systematic, a teacher of generations of undergraduates and research students—at Trinity College, Cambridge, while Young, a retired civil servant, maintained a connection with All Souls, Oxford. Quite a few of the allusions—mostly quotations—eluded even Kitson Clark, his assistants, and consulted experts. But the great majority were run to ground. And it turned out that *Portrait* is full of errors! There are dates that are off by a year or two, quotations that are wrongly given, and material alluded to by Young and investigated by Kitson Clark that fails to bear out Young's interpretations. Along the way Kitson Clark provides much fascinating information and, in the manner of the brilliant supervisor

that he was, he also provides the direction and the beginning bibliography with which the student can explore Young's insights. Kitson Clark's annotations enrich our enjoyment of one of the most remarkable historical essays of our century; they have the added virtue of enriching our understanding of England in the nineteenth century.

PETER STANSKY
Stanford University

PETER ADAMS. *Fatal Necessity: British Intervention in New Zealand, 1830-1847*. New York: Oxford University Press. 1978. Pp. 308. \$23.00.

In this book Peter Adams produces a powerful and compelling argument in support of Prime Minister Lord Melbourne's resigned comment in 1837 that the situation in New Zealand was "only another proof of the fatal necessity by which a nation that once begins to colonize is led step by step over the whole globe." Melbourne maintained that it had become Britain's indispensable duty, for a variety of reasons, to intervene in New Zealand. On a superficial level such a judgment must seem a very convenient one for a colonizing power, but the importance of Adams's book is that it illustrates quite clearly the British government's extreme reluctance to become involved in an extension of formal empire in the Pacific. Adams also shows that on this occasion the Whig government was out of step with both the missionary societies and the Wakefieldian New Zealand Association and its successor organization, the New Zealand Land Company. Both missionaries and Wakefieldians lobbied the government during the late 1830s and provided it with a copious supply of conflicting claims and information.

In many ways the lobbies were overtaken by the march of events on the New Zealand frontier; the disintegration of Maori society under the stimuli of European migration, religion, military technology, trade, and disease created a situation that forced Great Britain to intervene in an attempt to safeguard the interests of the native inhabitants and the settlers. The seeds of conflict of interest lay at the heart of this "dual duty" to protect Maoris from settlers and settlers from Maoris. Despite the genuine humanitarian concern for the Maoris that underlay the Treaty of Waitangi, the Victorian ideal of racial equality was a very flawed ideal indeed, as Adams succinctly points out; it embodied a deep-seated cultural exclusiveness that prevented recognition of any value in Maori society. As other New Zealand historians have pointed out, the combination of the "dual duty" and cultural chauvinism was to lead inexorably to the Maori Wars of the 1860s.

Adams has made use of a wide range of primary material, ranging from missionary society papers and private papers, to the usual array of parliamentary debates and official government sources relating to New Zealand. If the book has a major weakness it lies in this somewhat old-fashioned concentration on a limited range of sources. One reads nothing of newspaper opinion, aside from a brief reference to the *Spectator*, nor does one find any assessment of popular opinion outside official, company, or missionary circles. Moreover, Adams has not placed this most useful study within the wider framework of debate over whether this period of the 1830s and 1840s can be described as one when anti-imperial sentiment held sway in Britain. A lively debate continues among imperial historians on this point, and, although Adams seems to have material that would relate to it, he does not address himself to the question. The reader can only regret such a decision, as this most stimulating and interesting treatment could have been a useful case study of British attitudes to empire had Adams chosen to cast his net a little wider.

F. G. CLARKE
Macquarie University

M. W. KIRBY. *The British Coalmining Industry, 1870-1946: A Political and Economic History*. Hamden, Conn.: Archon Books. 1977. Pp. 278. \$17.50.

If there is an "English malaise" that afflicts advanced industrial economies—a malaise whose symptoms include hostile and strike-prone labor, inept management, inefficient production, sluggish wages, falling profits, and a declining competitiveness on the world market—then the British coal-mining industry is its classic example. Archaic, inefficient, disastrously turbulent, it has probably been the "sickest" segment of the twentieth-century British economy, yet a segment so vital that its sickness could not safely be left to cure itself. Hence the repeated attempts by twentieth-century governments to devise for it new and increasingly drastic cures. Even before 1914 the miners called for nationalization as the only effective remedy; but since no government before 1946 could implement it—although repeated crises of the industry required increasing measures of government planning and reorganization—public policy for the coal industry before 1946 consisted of repeated, *ad hoc* efforts to achieve coordination, amalgamation, market sharing, and rationalization of the industry by methods short of outright nationalization. Such experiments in reluctant collectivism, often carried out by Conservative governments favoring a reduction of public controls, provide striking examples of the ways in

which the characteristics of (future) nationalized industries were worked out piecemeal before the advent of nationalization itself.

This is one of the promising themes of M. W. Kirby's well-researched study; others are the surprising continuity in government policies for the coal industry—a reflection of deeper continuity in political mentalities and social constraints—and the insolubility of the industry's problems within a free-enterprise framework. Unfortunately, Kirby's immensely detailed survey of the relations between the coal industry and government prior to 1946 only occasionally touches on these themes, and they remain underdeveloped. Though he takes up, one by one, the major crises of the industry and the government's responses, analyzes political maneuvering, summarizes legislation, and passes judgment on its effectiveness—though the separate pieces of his study are competently done and contain some interesting judgments—the whole is not bound together by any coherent arguments. It has no identifiable theses and reaches no general conclusions.

The value of the book, therefore, lies chiefly in its piecemeal reassessments of well-known policies and events. These include a defense of the government's handling of the Sankey Report and of Baldwin's handling of the miners in 1926, a strong sympathy for the dilemmas of Conservative politics, and an analysis of the contradictions of the Coal Mines Act of 1930. Yet even the best parts of the book are seriously marred by an unnecessarily tedious prose style. (Kirby is heavily addicted to phrases like "it should be born in mind that.") The narrative meanders distractingly; many paragraphs lack topic sentences and internal continuity; and details are piled up relentlessly without any clear indication of what they mean. In short, this is a dissertation that has not yet been fully transformed into a book.

WILLARD WOLFE
Bowdoin College

STUART A. COHEN. *British Policy in Mesopotamia, 1903-1914*. (Monograph, number 5.) London: Ithaca Press, for The Middle East Centre, St. Antony's College, Oxford. 1976. Pp. 361. \$16.00.

PETER SLUGLETT. *Britain in Iraq, 1914-1932*. (Monograph, number 4.) London: Ithaca Press, for The Middle East Centre, St. Antony's College, Oxford. 1976. Pp. 363. \$16.00.

British involvement in Iraq, then called Mesopotamia by Europeans, intensified between 1903 and 1932. With the publication of these two monographs by the Middle East Centre of St. Antony's

College, Oxford, a student of the era and area can begin to clarify the nature and impact of European influence in these isolated former Ottoman provinces. Stuart A. Cohen's study is narrowly focused on the evolution of British policy toward the Tigris-Euphrates Valley, while Peter Sluglett is concerned with the impact of direct British control, particularly its effect on the peoples and institutions of Iraq.

Cohen's *British Policy in Mesopotamia, 1903-1914* focuses entirely on the process of policy making toward Iraq during those years and deals with the mythical "official mind." The book has three sections, covering the development of a Mesopotamian policy after 1903, its implementation through 1910, and its modification up to 1914. Cohen asserts that "British policy towards Iraq was determined almost exclusively by the preoccupations of the Foreign Office" (pp. 320-21). His case might be stronger had he used other available sources, particularly the archives of the *Times*. The Foreign Office did not operate in a vacuum, and what is missing from this study is a sense of the impact on imperial policy of personalities and of the received opinions of that era. There is much too much jargon, and the book's organization tends to obscure the process described. The footnotes need editing and consolidation so as to be more informative and usable. The author's use of initials for the first names of almost all the figures mentioned in the text and index tends to cast doubt on his familiarity with his subject. One example will suffice: the J. Gorst listed in the index happens to be John Eldon Gorst the younger, but as J. Gorst he could easily be confused with his father, the prominent politician. All in all, though, the pattern of British interests, knowledge, and ignorance about Mesopotamia is there for the interested reader to discern and ponder.

In *Britain in Iraq, 1914-1932*, Peter Sluglett skillfully analyzes the era that Elizabeth Monroe has labeled "the years of good management." During the few years between the wars the British found, after a false start (an attempt at direct rule on the Indian model), that they could maintain their influence in Iraq by propping up elements of traditional elites, playing off the minorities, and maintaining a small, air-mobile military establishment and some few judiciously placed officials. This regime was cheap, effective, and even allowed for a facade of independence to be granted to Iraq in 1932.

Sluglett has explored a wider variety of official and unofficial sources than Cohen, including some Arabic materials and the Baghdad High Commission Papers now in the National Archives of India. The book is divided into two parts. The first discusses Iraq during the British invasion and occu-

pation and the creation of the mandate regime. In it the author covers the impact of the 1914-18 war, the Cairo Conference, and the vagaries of Anglo-Iraqi treaty making; he contributes an illuminating chapter on oil, the boundaries, and insolvency; and he treats the prelude to conditional independence in 1932. The second section of the book analyzes the effects of British intervention on land and tribal policies, defense and internal security, educational policy, and the immediate legacy of the mandate.

Unlike Cohen, Sluglett's sense of the setting, and of the individuals and groups involved, seems sure. In chapter three, entitled "Oil, Boundaries and Insolvency," he shows to be nonsense the official line that oil was not central to British official policy in Iraq. Sluglett writes that "new evidence . . . reveals beyond reasonable doubt that oil as much as strategic considerations dominated British official thinking toward Iraq. It is certainly true that Britain's subsequent interest in Iraqi affairs has approximated very closely to the interest of the Iraq Petroleum Company" (pp. 103-04).

Sluglett's study is the best yet on the genesis, development, and impact of British activities in Iraq between 1914-32. Oil and the route to India brought the British into Iraq. Once there, they played a ruthless game of divide and rule, propped up a Sunni minority, developed a land tenure and tax system that contributed to the enslavement of the peasant cultivator, used minorities—especially the Assyrians and Kurds—for their own purposes, and saw that the Iraq Petroleum Company had its own way. The policy described by the cynical Chief of the Imperial General Staff, General Sir Henry Wilson, as "hot air, aeroplanes and Arabs" worked in the short run. Its legacy, however, is not a happy one. Sluglett's words sum it up: "In any balance sheet for the Mandate, the Iraqi people, outside the small circle of government, insofar as they were affected by its activities, were the losers" (p. 297).

PETER MELLINI
Sonoma State University

MICHAEL FREEDEN. *The New Liberalism: An Ideology of Social Reform*. New York: Oxford University Press. 1978. Pp. xi, 291. \$24.00.

Time was when scholars thought pre-1914 English liberalism dead: dead as a party, dead from the neck up. Time was (and still is) when the test for any liberal was his groping—however incomplete—toward socialism as the higher truth. Such absurdities are powerfully challenged in this closely reasoned and convincing proof that liberal thought flourished, broadened, and, above all, modernized itself before 1914. Contemporaries

knew this. As socialists they might criticize (often well); as socialists they might welcome it; but they did not doubt it. Thanks to this important book, we may catch up with the contemporaries.

Who spoke for the new liberalism? L. T. Hobhouse, J. A. Hobson, Canon Samuel Barnett, and over a score more. What was their message? First, they affirmed as a matter of science as well as ethics that society was a community imbued with collective moral purpose of its own; a rebuke to socialist materialism and Manchester economics, this idea restored the moral function of the state and ended the isolation of economics from humanism and morals. Second, they asserted the duty of the state to secure not only liberty but welfare and "every other necessity for national progress," collective and individual (p. 55). Third, they desired a socially created capital, available for reform and controlled by the state. Hobhouse linked biology, ethics, and history; for him, progress toward more orderly cooperation—a matter of evolutionary and historic fact—demonstrated the rise of "collective humanity," self-determining and morally conscious (p. 48). Against exaggerated biologism new liberals affirmed the central place of mind, including the mind and talent of the "lower orders" (pp. 89-91): a double victory, for liberalism's old principle of rational choice and for new, democratically oriented, reform. The primacy of human purpose, the inseparability of moral values from "fact," the vital interdependence of all human life, infused all arguments.

Change was re-formulation but not repudiation; the new liberalism built on the old. Voluntarism, competition, gradualism, and practicability lived on, old principles cushioning the new. It was a work of synthesis and fulfillment. Two other contentions will raise eyebrows. First, no total break with *laissez faire* had to be made, for liberals had never been yoked to rigid *laissez faire*. In old and new, the ideals of liberty and welfare were "simultaneously predominant" (p. 53). Second, far from owing most new ideas to socialists, liberalism drew on independent sources, gave more to socialism than it got, and up to 1914 surpassed socialism in sheer intellectual importance and vitality (pp. 38-41). This will be challenged. All, however, ought to applaud his reminder that radicals, most socialists, and advanced liberals occupied then a broad common upland of aspiration and doctrine. He is bang right.

One asks whether theorists translated premises into program? A rich chapter on implementation is the answer. Did they face the special dilemmas of liberalism? How could they move liberals toward spending and regulation, yet keep middle-class support? Could they show workers that capitalists could be trusted, yet persuade enterprisers that welfare did not spell ruin? Michael Freeden

shows that liberals did not dodge this. Finally, how representative and how strong was this avant-garde? Freeden cites their dismay at "official Liberalism," but finds them strong enough to create the channels into which "the Liberal mainstream was flowing" (p. 5).

Side-glances abound: into the eugenics phenomenon, so repellent, so attractive; into Beveridge's illiberality regarding the unemployed; into the contrast with Bismarck's reformism; and into origins—on which, we hope, Freeden will give us more. We shall be talking about this book for a long time.

PAUL BARTON JOHNSON
Roosevelt University

R. J. Q. ADAMS. *Arms and the Wizard: Lloyd George and the Ministry of Munitions*. College Station: Texas A & M University Press. 1978. Pp. xv, 252. \$13.50.

David Lloyd George's alleged place in history as "the man who won the war" of 1914-18 may rest more firmly on his accomplishments as munitions minister than as premier. This is the implied judgment of R. J. Q. Adams's brief study, based chiefly upon intensive research into the papers of the short-lived wartime department created, fashioned, and dominated by the hyperactive "Welsh wizard."

Adams documents the familiar but one-sided story of how the Welshman and his cohort of "push and go" industrialists, scientists, and statisticians outflanked the traditionalism and inertia of the War Office's Ordnance Department. According to this account it became apparent early in the war that Von Donop, the stubborn master general of ordnance, would not sanction munitions production of the amount and kind needed by a continental-size British army fighting an unprecedented type of war. Lloyd George decided that he was the only politician capable of untangling the red tape and delivering the goods to the men in the trenches. The author acknowledges that the then chancellor of the exchequer, bored with his old job, was the only Liberal minister willing—indeed eager—to impose "war socialism" upon the country. Drastic mobilization of manpower and resources was at odds with orthodox Liberalism and repugnant to many Liberals. The Munitions of War Act of 1915 gave Lloyd George the powers he needed to become Britain's industrial dictator after the May crisis and the formation of the first coalition.

Adams accepts the conventional view that Lord Fisher's resignation, and not backstairs intrigue, brought down the last Liberal government. Lloyd George is seen as adroitly exploiting both the political crisis and the "shell scandal" manufactured

by the Northcliffe press to make himself master of armaments production. During the year he headed the munitions ministry the Welshman greatly aggrandized his powers, mostly at the War Office's expense. Beginning only with manufacture of guns and ammunition, the new ministry soon acquired authority over research, design, and supply, enabling it to develop machine guns, the tank, amatol, poison gases, and other lethal weapons the old-fashioned Ordnance Department had been reluctant to promote. Adams accepts at face value the claim of Lloyd George and his lieutenants that only their determination to pragmatically experiment and ruthlessly produce saved the country from disaster.

Useful and interesting as this book is, it would have been better if the author had analyzed the motives and methods of Von Donop and his War Office colleagues instead of writing them off as hidebound obstructionists. Adams agrees uncritically with Liddell Hart and other standard military historians that there was a generation gap or culture lag between the psychology of the generals and the military technology of the twentieth century that was dimly perceived only by Lloyd George and a few others, mostly civilians.

This study complements but does not replace the official, twelve-volume *History of the Ministry of Munitions and The Great Munitions Feat*, edited in 1921 by G. A. B. Dewar, a journalist critical of Lloyd George. Yet it earns a place beside Paul Barton Johnson's *Land Fit for Heroes* as an important inquiry into the administrative politics of the First World War.

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ANNE ORDE. *Great Britain and International Security, 1920-1926*. (Royal Historical Society Studies in History.) London: The Society. 1978. Pp. viii, 244. \$16.00.

This revised doctoral dissertation, based on British, German, and French archival materials, is a detailed, scholarly study of selected aspects of early post-World War I British foreign policy. The primary, but not exclusive, focus is on European security issues arising from the Paris peace settlement. Anne Orde begins with a discussion of Lloyd George's protracted negotiations with the French in 1921-22 to provide a security pact against the defeated Germans as a substitute for the lapsed 1919 guarantee treaties. These talks failed, largely because of the two powers' divergent attitudes toward European reconstruction and the British unwillingness to sign a binding military convention. Lord Robert Cecil, in his capacity as a "peace expert," presented the more ambitious

draft Treaty of Mutual Assistance to the League of Nations Assembly in 1923: this was designed to strengthen the League Covenant and to link general security with armaments reduction. Cecil's scheme met with unanimous British departmental opposition, as well as rejection by the Dominions, because of their narrow view of League functions. The similarly idealistic Protocol for the Pacific Settlement of International Disputes (Geneva Protocol), sponsored by MacDonald and Herriot in 1924, was unanimously recommended by the League Assembly, but it aroused the opposition of the British service representatives, the dominion delegates and, most important, the cabinet. It would have required far-reaching amendments to exclude automatic sanctions even had the Labour government remained in office. Austen Chamberlain, the new Conservative foreign secretary, while favoring a close Anglo-French relationship, rejected any broad international involvements, especially in the tangled affairs of Eastern Europe. Instead, he approved and won cabinet endorsement of a limited four-power western regional security proposal, first proposed by the Germans in 1923. After lengthy negotiations, complicated by French insistence that Germany sign arbitration pacts with France's eastern allies and by the Germans' anxiety to maintain their 1922 treaty ties with Soviet Russia, the Locarno agreements were finally initialed on October 16, 1925. Locarno improved great power relations in Europe, at least temporarily. "The gain for Britain was release from constant involvement in Franco-German frictions" (p. 210).

This story has been told previously by historians of British and European interwar diplomacy and by biographers (and in memoirs) of leading statesmen of the period, but the reader will find here a fresh perspective and many interesting new details. Orde's main point is that the British cabinets of the 1920s, despite prescient warnings from some foreign affairs experts, were primarily concerned with the defense of the home islands and the empire and viewed European stability as only a secondary interest. She also notes that British military planners, in the absence of any major international commitments and for economic and other reasons, made correspondingly limited security preparations. In a very concise but important chapter, she discusses the strategic role assigned to the armed forces, the ten-year rule, the attempts to coordinate defense measures with the Dominions, the Washington Conference agreements, the new Far Eastern problems which led to construction of the Singapore naval base, and the vulnerability of India.

To sum up: this is a well-conceived, informative, and carefully documented monograph which will

be useful to students of recent British and European history.

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PENELOPE HETHERINGTON. *British Paternalism and Africa, 1920-1940*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1978. Pp. xvi, 196. \$22.50.

The two words "paternalism" and "Africa" in the title of this important book need qualification. Penelope Hetherington, a staff member of the University of Western Australia, has undertaken a survey of British concepts and attitudes toward indigenous peoples in the British possessions of Africa between the wars. She bases her description almost exclusively on books and articles written by British scholars, journalists, missionaries, administrators, and politicians during the period. Paternalism was the underlying theme within this body of writing—and in that sense the word is descriptive of the content—but there were many other concepts and also a radical fringe that was not paternalistic. Also, paternalism in this book includes only concept, not practice, as one could infer from the word alone. Accomplishing most of her research in Australia, where she had only limited access to the primary sources of British administration in the African protectorates and colonies, the author made no attempt to describe the formulation and application of actual policy or the African response. Moreover, her study concerns British attitudes not toward Africa, as the title implies, but toward the African peoples—their race, culture, and potential.

Within its context, which the author clearly defines in the introduction, the book makes a weighty, two-fold contribution to British imperial history as it concerns Africa. In one sense it is a valuable reference book on the authors, journals, and organizations in Britain interested in Africa between the wars. In another, it is an astute, objective analysis of the ideas that then characterized the intelligentsia's conception of Britain's role in Africa. The book is divided into two parts, the first consisting of two chapters in which the writers, periodicals, and institutions are described, the second embracing six chapters on categories of concepts. The several chapter headings of part two reveal the content: the meaning of colonial trusteeship; the problems of social change; theories about race, development, and research; and education and administration.

Essentially there were three schools of thought. The conservatives, led initially by Frederick Lu-

gard, emphasized Britain's civilizing and protecting mission, the dual mandate, indirect rule, laissez-faire development, and social evolutionary theory. Ultimately Lord Hailey achieved a new conservative synthesis by rejecting Lugard's indirect rule and urging the training of Africans for self-government. Liberals such as J. H. Harris, W. M. Macmillan, and J. H. Oldham accepted Britain's civilizing mission but were critical of its record in African development and stressed the need for state action. Most radicals, including Norman Leys, Leonard Barnes, and Bronislaw Malinowski, were even more critical. Supporting theories of Marx, Lenin, and Hobson and of functionalist anthropology, they sought an end to colonial rule as soon as a systematic attack could be made on disease, malnutrition, ignorance, and poverty. An extreme radical group led by R. Palme Dutt rejected even this form of paternalism and held that colonial rule could terminate only through African rebellion.

There are only two significant flaws in this book. The index, which obviously was not compiled by the author, omits entirely the individuals, organizations, and ideas contained in the copious reference notes appended to each chapter. Secondly, the conclusion, limited to five pages, does not at all provide the synthesis needed for this important subject.

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ARTHUR L. SMITH, JR. *Churchill's German Army: War-time Strategy and Cold War Politics, 1943-1947*. (Sage Library of Social Research, number 54.) Beverly Hills, Calif.: Sage Publications. 1977. Pp. 158. Cloth \$14.00, paper \$6.95.

This thin detective monograph proposes yet another cause for the friction between the Anglo-American nations and the USSR during the last stages of World War II and its importance for the postwar period. It also attempts to provide a quick and authoritative guide to the German surrender. Such ambition is admirable, but this particular book will not serve its stated purpose.

Since many of the documents that Arthur L. Smith, Jr. needs have either been destroyed or remain untouched, he has advanced a cloudy hypothesis: Churchill became so alarmed by the prospect of Soviet expansion in May 1945 that he instructed his commanders to preserve carefully German arms and even German soldiers in order to arrest the Bolsheviks' advances—a somewhat new manifestation of a familiar theme.

The least satisfactory part of the book is a cursory review of the difficulties preceding and follow-

ing the promulgation of the unconditional surrender doctrine during the January 1943 Casablanca conference and the inherent difficulties concerning the strategic decision to have an early second front in northern Europe. Relying on the judgments of secondary authorities rather than on substantial documentary evidence, Smith seems too willing to accept as true the notions that the Soviets immediately became convinced of their ability to pursue an aggressive policy following their victory at Stalingrad and that Churchill, unlike his American counterparts, underwent some sort of sudden conversion to a more anti-Soviet policy following the Teheran conference.

Skipping blithely over 1944, Smith presents an interesting description of the armistice negotiations at Bern, the patchwork surrenders along the Western fronts in April, and the last-minute rush by the Anglo-American forces for tactical and political advantage while being abetted by the Germans. According to this line, the British were more active and eager for Doenitz's collaboration than were the Americans. While he notes the confusion surrounding the surrenders in May at Reims and Berlin, the author does no better than the rest of us have done in explaining why the Western commanders initially ignored the work of the European Advisory Commission, which had been negotiating a four-power surrender instrument fitfully since the beginning of 1944. The result of all this activity was to "deliver" to Montgomery nearly three million German prisoners of war in May 1945 who might otherwise have been taken by the Soviets.

Smith's argument that Britain's "German army" continued to bedevil Anglo-American and Soviet relations for several years following the war is interesting but like the rest of the book contains more shadow than substance. The details cannot be ignored and a subject so large, important, and complex cannot be treated satisfactorily in fewer than one-hundred-fifty pages of text.

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KENNETH RICHARDSON. *The British Motor Industry, 1896-1939*. With the assistance of C. N. O'GALLAGHER. Hamden, Conn.: Archon Books. 1978. Pp. xiii, 258. \$17.50.

Here is an extensive but rather shallow account of many aspects of the automobile business in Britain to 1939. Kenneth Richardson, senior lecturer at Lanchester Polytechnic in Coventry, and his research associate, C. N. O'Gallagher, devote about

half their space to an account of the growth of the industry from the 1890s to the Second World War, anchoring it on brief discussions of many firms and businessmen. Then they cover racing, sports cars, roads and traffic regulations, hire purchase, the motoring press, petrol distribution, repair garages, and auto dealerships. The book ends in the middle of a paragraph about the life of a Birmingham car dealer. There is no conclusion.

Among the interesting subjects discussed, Richardson devotes nine pages to the English Daimler company but wisely does not offer yet another account of Rolls-Royce. He also has useful discussions of specialist body manufacturers, of Austin in the 1920s, and of some component makers. The Ford and General Motors experiences in Britain, however, are almost completely ignored, as are developments in Scotland. The footnotes contain helpful clues to the location of manuscript business records of some companies and hard-to-find articles in the trade press. These sources, together with secondary accounts and interviews with persons once associated with the industry, make up the evidence used.

This study is not economic history, for there is no sustained economic or financial analysis of the automobile industry and British failures and successes with it. Richardson seems to think that his brief discussions of a great number of people—mostly businessmen—make this work social history, but he does not attempt to develop patterns among them. One finds almost nothing on labor or the labor movement and only a few details on changes in technology or management methods.

Empirical to a fault, Richardson is loathe to tie his people and his facts together with generalizations. The book will be a helpful introduction to those first coming to the subject, but the lack of explanations for major trends and the absence of international comparisons will limit its usefulness for general historians, while specialists in automobile history will find little new, save for some bibliographical citations.

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DAVID STEVENSON. *Revolution and Counter-Revolution in Scotland, 1644–1651*. (Royal Historical Society Studies in History.) London: Royal Historical Society. 1977. Pp. xi, 283. \$15.00.

David Stevenson has completed his account of Scotland's time of troubles that he began with *The Scottish Revolution, 1637–44: The Triumph of the Covenanters* (1973). His new book maintains the high level of scholarship of the previous work; those seeking a thorough and dispassionate narrative of

Scottish political history in the period 1644–51, solidly based on the official sources, need look no further.

Stevenson begins with the Scottish intervention in the English Civil War in 1644 to preserve the political and religious gains of their almost bloodless revolution, to which the king had submitted in 1641. Stevenson's Scottish revolution was an aristocratic victory that increased the power of the estates at the expense of the king; the kirk and other groups are portrayed as distinctly junior partners. All parties recognized that security for their revolution depended upon guarantees from England; disagreement over whether king or parliament could best supply those guarantees led to the civil conflict that ravaged Scotland until the Cromwellian conquest.

Although Montrose's campaigns delayed a revival of royalist sentiment among the nobility, the Engagement of 1648 revealed that a majority of the aristocracy, many of them Covenanters, could not avoid seeking accommodation with the king. Stevenson treats the engagement as a counter-revolution. No doubt the king and the Duke of Hamilton intended one, but moderate covenanting noblemen supported this ill-fated scheme as a method of preserving the revolution that they had helped to lead. As always, the terms revolution and counterrevolution are tricky in a seventeenth-century context.

With the failure of the engagement the revolution now began a more radical phase dominated by extremists of the kirk who depended upon the lower orders of society for support. This political base was inadequate in a society so dominated by its aristocracy; only Cromwell's invasion prevented successful royalist and aristocratic revival around Charles II. Presbyterian writers have sometimes portrayed this period of kirk history as a golden age, but the fissures that now appeared in the structure of the kirk were never to be completely repaired. Stevenson demonstrates that 1644–51 revealed the failure of Presbyterianism, which proved too easily dominated by powerful laymen or too susceptible to capture by extremists.

Aside from the inevitable debate over what constitutes a revolution in the seventeenth century, this book should become the standard account of Scottish political history for the period. Stevenson has steadfastly refused to take the cudgels for either sectarian party and has managed to clarify Montrose's political significance without lapsing into romantic adulation. Lowland Scots feared and distrusted him because his Irish and Highland troops committed atrocities; his very success undermined the king's position in Scotland. Stevenson has also rescued the Scots from the worst abuses of contemporary English propaganda. If

the Scots made undesirable allies, consider the king and the English Parliament! The result is less heat and considerably more light cast upon a neglected period. Finally Stevenson points the way to new research—particularly on the social background to the covenanting radicals of the south-west. After all, as the aristocratic politicians realized, it was here that real revolution was brewing.

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MICHAEL FLINN *et al.*, editors. *Scottish Population History from the 17th Century to the 1930s*. New York: Cambridge University Press. 1978. Pp. xxv, 547. \$39.50.

Scotland has been poorly represented in the expansion of demographic studies published during the past decade. This book fully justifies such reticence. Instead of rushing into print with premature assertions, a string of dubiously typical microstudies, or an exhortatory statement of what *needs* to be done, the authors have quietly and with superb management and planning coordinated a research project on a very large scale and have held their fire until the exercise was completed. The outcome is a model of aggregative analysis that all researchers will find impressive and instructive.

The term "aggregative analysis" is stressed, for Scottish parish registers are too few and defective in quality to permit family reconstitution. The solution the present authors adopt is to focus primarily on the region rather than the individual parish, employing a varying mix of surviving registers to construct a profile of demographic trends (more particularly, mortality fluctuations), supplementing this evidence with extensive reference to contemporary comment, kirk session records, and civic and government papers. The authors handle the material with considerable methodological ingenuity and admirably sane and critical judgment. The result is not mere population history but a social and economic study of a very high order.

The six authors (with occasional contributions from specialist outsiders) share responsibility for the whole book. This makes for a consistency of approach and interpretation normally lacking in works of collective scholarship. Broadly, they attribute stagnation in the seventeenth and first half of the eighteenth centuries to major famines and epidemics; recurring every twenty to twenty-five years, these disasters reduced population growth to insignificant rates, although the evidence presented is not wholly convincing (pp. 182, 246). Tentatively they attribute renewed expansion

thereafter primarily to a decline in the frequency or virulence of infectious disease (pp. 16, 248) and the disappearance of famine.

In common with English historians they cannot adequately explain why these crises diminished in frequency and effect after ca. 1741. With regard to the reduced incidence of famine, they suggest that developments in agricultural techniques played a negligible role compared to the extension and improved organization of the market economy and the increased efficiency and humanity of relief agencies (pp. 8–13).

Growth in the later eighteenth century seems to have been markedly slower than in contemporary England and Ireland, only accelerating sharply in the 1800s with the diffusion of smallpox vaccination. The authors consider inoculation to have had scant effect (pp. 14, 390–93).

The potato probably only became a dominant element in the Highland cottier's diet in the early nineteenth century. Its role was thus supportive, enabling a growing population to be accommodated and delaying by half a century the approach of a Malthusian crisis (pp. 28–38, 421–38)—rather than, as in the traditional interpretation of Ireland's history, acting as a direct stimulus to population expansion.

In these areas *Scottish Population History* should provoke thought among English and Irish historians as well as among Scots, although it is fair to point out that, as the authors readily admit, the emphasis given to the death rate in preindustrial times is partly a residual of the virtual absence of usable fertility data before the nineteenth century. Thus the chapter covering the seventeenth century is a masterly assessment of mortality crises. But should historians ever come to suspect that the key to the cessation of sixteenth-century population growth lies in changes in nuptiality or fertility, they will find few pointers here to guide them.

With the nineteenth and twentieth centuries the expansion of government statistics inevitably diminishes the authors' scope for reinterpreting the past, though their treatment of illegitimacy and of internal migration are of particular note (a final chapter being devoted to population movements from the later eighteenth century onwards).

There are points, few and relatively unimportant, where the authors' judgment is questionable (for example, over the significance of data presented on pp. 183–85, 280, or 286–88). But the overall soundness of research and interpretation is transparent. Moreover, the general clarity of exposition and meticulous discussion of sources will make this study an essential reference work for future researchers. It is simultaneously pathfinder and textbook.

The authors have clearly benefited from the

work of previous scholars. But nothing should detract from their achievement. This is, quite simply, the most important contribution to historical demography to have emerged from the British Isles.

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DESMOND BOWEN. *The Protestant Crusade in Ireland, 1800-70: A Study of Protestant-Catholic Relations Between the Act of Union and Disestablishment*. Montreal: McGill-Queen's University Press. 1978. Pp. xiv, 412. \$23.95

Often when insiders write the history of a "firm," the result is long on anecdotes and short on skeptical insights. Desmond Bowen's book follows this pattern. Bowen is an Anglican clergyman who teaches history at Carleton University, Ottawa. He knows the Irish branch of the Anglican Church well, in part through having served several terms as a summer locum tenens for vacationing Irish rectors. His eye for clerical gossip concerning the Anglican clergy is shrewd, although considerably less sure regarding the Roman Catholics and the Presbyterians. Thus, we find witty vignettes, such as those concerning the inept Hon. Edmund Knox, a younger son of Viscount Northfield, who was raised to an Irish bishopric and whose "manner of concealing his ineptitude was to spend most of the years he was Bishop of Limerick, 1834-49, on the continent." And the case of the Hon. Percy Jocelyn, third son of the Earl of Roden, is noted: he had been Bishop of Clogher for two years when he was caught in flagrante delicto with a guardsman in the White Hart tavern in London.

Unhappily, Bowen lacks a cold and skeptical eye. He has immersed himself so thoroughly in the controversial religious literature of his period that he tends to be much too credulous in describing the work of the Protestant evangelizers: he does not quite take them at face value, but almost. The result is that although he presents a convincing picture of the proselytizers' minds and of what they thought they were achieving, he does not come to a realistic assessment of what actually was going on. In particular, he sees the aggressive evangelicals as constituting a much more important and much more coherent phenomenon than was the case.

This occurs, initially, because of Bowen's protean concept of "Protestant." In nineteenth-century Ireland the term usually referred to members of the Established (that is, Anglican) Church, and in most cases Bowen concentrates upon them. Sometimes, however, he slides his focus and discusses Presbyterians and Methodists under the

Protestant rubric. Concomitantly, Bowen over-emphasizes the importance and the cohesion of the evangelical element within the Anglican Church. He refers to the "prevailing Evangelical theology of the Church of Ireland" and then later suggests that there was a "prevailing High Church ethos in the Church of Ireland" with which the evangelicals merged to form a "High Church Evangelicalism" that allegedly dominated the Anglican Church before disestablishment. Leaving aside both the apparent contradiction in Bowen's two statements and the question of what the neologism "High Church Evangelicalism" means, the fact is that Bowen does not at any point prove that evangelicalism prevailed among Irish Anglicans. Indeed, a case can be made that it was a minority viewpoint: only a small minority of the bishops were evangelical before 1870, and the bishops controlled the right of appointment to most of the parishes in Ireland.

Thus, a false causality prevails in this work, evangelicalism often being presented as a major independent and disruptive force in Irish history. Bowen holds the extraordinarily romantic view that "when the two peoples of nineteenth-century Ireland were left alone to work out their religious and cultural relationships, they accepted the inevitability of living in a pluralist society." At some points he suggests that Archbishop Magee's infamous anti-Catholic charge of 1822 began a series of religious controversies and subsequent social fractures which did not subside until after disestablishment in 1870. Similarly, the author says that, without the crusading of the Reverend Alexander Dallas, it is doubtful whether Paul Cullen could have carried through his mid-century reorganization of the Irish Catholic Church along Ultramontanist lines. (Admittedly, Bowen in places appears to contradict himself by suggesting—accurately I think—that the evangelicals' activities did not in themselves cause major religious problems, but instead merely helped unleash tensions that were engendered by Ireland's undergoing radical changes in basic political, social, and economic structures.)

This brings us to the crucial point: it is not at all clear from the evidence presented in this volume that there actually was a Protestant crusade. During the middle decades of the nineteenth century, far from striking out to convert the Catholic Irish, most Anglican clergy and clerical administrators were conducting an ordered retreat. The internal efficiency of their church, its financial structure, and its delivery of pastoral services to the Anglican population were greatly improved at the same time that the role of the church in Catholic-dominated Irish society diminished. Perhaps instead of a Protestant crusade led by aggressive evangelicals,

Bowen would have done better to present his study as a series of unfortunate alley fights between religious enthusiasts.

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PAOLO ALATRI. *Parlamenti e lotta politica nella Francia del Settecento*. Rome: Laterza. 1977. Pp. viii, 532.

Paolo Alatri, professor of modern history at the University of Messina, has published books on nineteenth- and twentieth-century Italian history and has also written a two-volume *Lineamenti di storia del pensiero politico moderno*. In recent years he has become deeply interested in the Enlightenment in France and the complicated social, intellectual, and political struggles of the last century of the *ancien régime*. This interest has manifested itself in numerous articles, review essays, and now the volume at hand, which is noteworthy for its subtle and judicious analysis of the social and political crosscurrents of an age in the grip of change and yet uncommonly resistant to it. No doubt Alatri's experience as a former member of the Italian Chamber of Deputies has trained him to be alert to the nuances and anfractuositities of complicated political situations.

Admirably conversant with the research of his predecessors in the study of the constitutional, administrative, intellectual, and social aspects of French history in the eighteenth century, Alatri emphasizes that it is practically impossible to understand the dynamics of eighteenth-century French politics without being fully aware of the role played by the *parlements*. Part two of the volume is a detailed and very readable chronicle of the political history of France from 1715 (when the *Parlement* of Paris won the right to register royal decrees before they could be put into effect) until the Revolution, and it illustrates the foregoing hypothesis. This chronological account is carefully prepared for by part one, a series of analytical chapters on the legal, administrative, and financial structure of the monarchy in the eighteenth century. These chapters are a *tour de force* of synthesis, discussing the personnel, functions, and machinery of the judicial system in France; the history of parliamentary remonstrances in the reigns of Louis XIV, Louis XV, and Louis XVI; the make-up of the councils of the central government; the ministers and other high officers of state; the *intendants*, the governors of provinces, and their relations with the local *parlements*; the provincial estates; the world of financiers and tax farmers and its relations with the *noblesse de la robe*; the sale of offices, which, though at first a way of attaching the bour-

geoisie to the monarchy, became in the course of the eighteenth century one of the greatest obstacles to reform; and, finally, the fiscal administration and attempts at reform. All of this is ably organized and very lucidly presented, an analysis rare for its comprehensiveness and judiciousness. The author has researched extensively in the Bibliothèque Nationale, besides being familiar with the printed literature, and includes in one of his appendixes (pp. 500-05) his translation of an unpublished pamphlet, *Catéchisme des Parlemens* (first months of 1789), which he found in the Bibliothèque Nationale.

Vacillating and ineffective though the monarchy was in the reigns of Louis XV and Louis XVI, it was not so much the enemy of reform and the cause of its own downfall as were the *parlements*. Their perquisites and privileges made them insuperably repugnant to registering and enforcing laws that incorporated any tax reforms. Witness the fate of Machault's *vingtième* in 1749 and, ultimately, of Maupeou's "coup d'état." The *parlements*, while posing very successfully as patriots and as enemies of "despotism," were in fact obstructionists who did their best to make reform inoperable, so that at last the regime sank into the apathy and hopelessness of imminent bankruptcy. As the author remarks, "La questione della fiscalità è centrale nella storia della Francia d'*ancien régime*" (p. 277).

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SAMUEL F. SCOTT. *The Response of the Royal Army to the French Revolution: The Role and Development of the Line Army, 1787-93*. Oxford: Clarendon Press. 1978. Pp. vi, 243. \$24.95.

Armies often reflect the problems and conflicts of the societies that create them. The French army from 1787 to 1793 was no exception. Samuel F. Scott has produced a work that traces the impact of the revolution on the line army. He also describes the role of the army and its impact on the course of the revolution. Finally, he discusses the transformation of the royal army into an instrument that loyally served a revolutionary republic.

The book begins with a detailed and useful description of the structure and composition of the royal army in the years immediately preceding the revolution. Scott avoids oversimplification and deals effectively with the complexities of the pre-revolutionary army. He notes that tensions between aristocrats and commoners were reflected in the army and that the aristocratic officers sympathized with noble resistance to the crown's at-

tempts at fiscal and political reform. Louis XVI's lack of confidence in his officers was an important factor in convincing the monarch to summon the Estates General.

In 1789 growing distrust between officers and enlisted men, resulting from conflicting attitudes toward the revolution, convinced the officers that they could not rely on their troops. This assumption, which was not in fact totally correct, led the king to believe that he could not defeat the revolution with military means. After July 1789, additional plans to use the army against the revolution also collapsed because counterrevolutionaries simply could not count on the army's support.

The author also points out that attitudes toward the revolution were not simply a function of status. Many officers supported the revolution while a fair number of enlisted men, especially in the cavalry and foreign regiments, adopted a counterrevolutionary stance. These internal divisions paralyzed the armed forces, thus depriving the crown of the ability to use force against its enemies.

Discipline declined drastically in 1790 and 1791, primarily as a result of continuing conflict within the army. Contact with civilians and revolutionary ideas intensified existing conflicts. The line army, however, managed to restore discipline. Officers and men alike came to support the new regime. Those who did not, emigrated. By 1792 the army had become a politically reliable instrument.

Scott has come up with no startling revelations, but he has effectively filled in many details. For example, his discussion of the Nancy mutiny of 1790 is one of the best available in English. The author has recognized, moreover, the complexity of the problems surrounding the nature and role of the army in a revolutionary situation. Thus, he has produced a useful book.

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FRANCES MALINO. *The Sephardic Jews of Bordeaux: Assimilation and Emancipation in Revolutionary and Napoleonic France.* (Judaic Studies Series.) University: University of Alabama Press. 1978. Pp. viii, 166. \$11.50.

Frances Malino's book is valuable to students of both the early modern state and Jewish history. By combining Arthur Hertzberg's analytical framework with Zosa Szajkowski's scholarly tradition, she achieves a work that is more accurate than that of the former and broader in scope than that of the latter.

Malino's thesis, that the accommodation of the Bordeaux Sephardic Jews to Christian society provided an important precedent for the terms upon which other Jews would later be accepted by the

modern nation-state, seems pedestrian at first sight. But the author redefines this theory, carries it further, and provides more substantial documentation than did its previous exponents. Her narrative, ending with the enactment of special legislation on March 17, 1808, leads to an analytical framework within which we can better understand the marginality (or perhaps one might say *luftmensch* quality) of those nineteenth-century European Jews who retained some religious identity.

In the course of this work Malino provides interesting insights, and her new documentary evidence illuminates important episodes. For example, her careful and original study of the Bordeaux community reveals that the January 28, 1790 law merely confirmed the Bordelais Sephardim's pre-existing privileges. Thus, no matter what it meant for other Sephardic Jews, for the Bordelais this law was the culmination of prerevolutionary developments rather than the inauguration of a new era. Malino also sheds new light on both revolutionary emancipation and the forces that created the modern absolute state by showing how the familiar dichotomy between French motives for enfranchising the Sephardim (practical, economic) and the Ashkenazim (revolutionary logic) applied to Napoleonic policy as well. Finally, her new documentation provides interesting insight into the operation of the Imperial Council and Napoleon's relations with his ministers.

A researcher interested in only one segment of the three-hundred-year period covered here will find it difficult to read this book selectively. Not only is it so tightly organized that the later chapters lose significance if read alone, but assumptions made in the earlier chapters acquire credibility (and, in part, comprehensibility) only in the later chapters. This weakness is partially caused by the author's reliance on outdated or questionable sources for general French history. For example, when she first discusses Sephardic religious commitment, her statements are unconvincing and appear to be contradictory. Her sources for the revolutionary debates are second-rate, and she uses Grossclaude's work on Malesherbes without indicating the controversy surrounding that book. (Or did she use the unexpurgated edition? If so, she should say so.) One wonders if some of her conclusions are colored by too heavy a reliance on the experience and reactions of Abraham Furtado. Still, after reading her entire book, the total weight of her evidence clarifies what appears unsatisfactory in the first few chapters. I heartily recommend this book to students of the Jews and the modern state with the proviso that they read the entire work.

RUTH F. NECHELES (JANSYN)

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JEAN-CLAUDE PERROT. *L'âge d'or de la statistique régionale française (an IV-1804)*. Paris: Société des Études Robespierriennes. 1977. Pp. 235.

Jean-Claude Perrot has collected and annotated a bibliography of both published and unpublished material on descriptive statistics of topography, economics, population, and society for each of the 130 departments of the Empire. The information included ranges from the end of the *ancien régime* to 1833, when the Bureau of Statistics was refounded, and, although there are not many sources on the non-French departments, the collection for the 86 French departments is rich. The 170-page bibliography is preceded by a concise and worthwhile introduction that explains the background of this era and the reasons why a "golden age" developed.

Perrot argues that the long-standing desire of rulers to know their subjects, the more recent ideals of the Enlightenment, and the immediate opportunity provided by the Revolution, prompted the Directory's minister of interior, François de Neufchâteau, to undertake important innovations. Perrot convincingly demonstrates Neufchâteau's responsibility for the *statistiques des préfets*, but his attempt to disprove Bertrand Gille's contention that the main impetus for regional statistics came under the Consulate is undermined when he credits Jean-Antoine Chaptal for the critical ingredient—the willingness of the government to publish the material collected. Perrot discusses both the administrative and personnel aspects of collecting and publishing statistics, and he highlights Jacques Peuchet's crucial aid to Chaptal, especially in the development of methods. Since this golden age was based on cooperation between volunteers, who collected the data, and the state, which organized the effort and published the results, the era ended when such cooperation ceased. In 1805 Napoleon returned to the traditional governmental policy of secrecy, and, when suspicions about the use of the data arose, collection soon ceased.

Although Perrot provides the essential information and sometimes more, as in his identification of the occupations of the volunteers, he tends generally to be somewhat superficial. He does not take his analysis of occupation very far, nor does he investigate the link between liberal ideology and the collection of statistics. He also fails to discuss why the French favored descriptive over arithmetical statistics. In short, the introduction could be more probing, but the bibliography is the real gold of this volume and everyone interested in the early nineteenth century should find it useful.

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GUILLAUME DE BERTIER DE SAUVIGNY. *Nouvelle histoire de Paris: La Restauration, 1815-1830*. Paris: Hachette. 1977. Pp. 525.

Guillaume de Bertier de Sauvigny's history of Paris under the Bourbon Restoration is a distinguished contribution to the splendid new series, the *Nouvelle Histoire de Paris*. Like the other volumes now in print it is an elegantly bound, beautifully illustrated presentation of the social, demographic, cultural, economic, administrative, and political history of the world's quintessential metropolis.

The author successfully combines a sense of the texture of Parisian life, through a shrewd selection of pertinent anecdotes and marvelously evocative illustrations, with a sophisticated analysis of a wide range of significant data illuminated by well-conceived graphs, charts, and maps. No one is better equipped than he to exploit the documentary riches of the Restoration archives.

The work is informed by the author's lifelong commitment to an understanding of the last Bourbon regime in its actual context rather than through the hostile hindsight of its liquidators. "As the Parisians chose to drive out Charles X," he writes, "it followed according to revolutionary logic that the repudiated regime had contributed nothing of value. Ever since, under the penalty of passing for an apologist of reaction and an enemy of the people, one has had to ignore everything the capital owed to the last of the Bourbons, their ministers, and their administrators" (p. 458).

To be sure, the royal government of Paris adopted the techniques of its despised predecessor, including the administrative anomaly of the overlapping jurisdictions of a Prefect of the Department of the Seine and a Prefect of Police of Paris. Bertier, however, persuasively documents the original and constructive contributions of the royal municipal administration in its assumption of the burden of policing, supplying, preserving, and amusing a vast, turbulent population in a period when the ability of a nineteenth-century city to concentrate people and proliferate refuse was not matched by an administrative technology adequate to the management of the masses or the disposal of the refuse. In housing conditions, public sanitation, and the pleasures of the populace, Restoration Paris was still reminiscent of a medieval city, but along with the 173 hectares of arable land within its walls it concentrated mills, factories, foundries, and the hundreds of petty manufacturing that contributed so much to the wealth of the community and the intolerable pollution of its environment.

The complexity and heterogeneity of the economy was reflected in the diversity of Parisian society. The very definition of Parisian has to take into account that fundamental division between a

stable indigenous citizenry growing slowly by natural increase and the fluctuating increment of an immigrant population constantly renewed by that "human sediment deposited by the flood of annual migration." This was the population that preserved its provincial peculiarities in the various *quartiers* of the capital, which concentrated and divided the populace according to geographic antecedent, occupation, and social class. Although Parisian neighborhoods were less segregated than they became in the second half of the century there was a more or less consistent sociogeographic demarcation separating those on the fortunate side of *l'aisance bourgeoise* from the impoverished majority whose condition ranged from *simplement malaisée* to *franchement misérable*. Bertier does not quite separate his Parisians into the two races portrayed in Louis Chevalier's *Classes laborieuses et classes dangereuses*, but his maps and charts, his statistics of disease, death, crime, and nuptiality and his descriptions of public pleasures and religious practices adumbrate the vast economic and cultural space between the privileged minority of the northwestern quadrant and the wretched masses in the working-class districts to the east and the slums along the Seine.

Given the limitations of space and the obligation to present a sweeping survey, this volume provides an impressive quantity of useful information and trenchant insight on the administrative, cultural, economic, and, above all, social history of the city. Perhaps because of the limitations of space the author chose to present the political history of Restoration Paris in the form of *Ephémérides politiques*. Such a political almanac was a popular form of political publication during the Restoration, but it is rather disappointing here because a simple listing of events is not a history of Paris as a political community and because controversial interpretations become the bland presentations of a chronicle.

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ABEL CHATELAIN. *Les migrants temporaires en France de 1800 à 1914: Histoire économique et sociale des migrants temporaires des campagnes françaises au XIX^e et au début du XX^e siècle*. In two volumes. Lille: Université de Lille III. 1977. Pp. 1,213. 160 fr.

In a breathtaking sweep across France over a two-century period, this remarkable work brings to the historical world (alas, posthumously) the fruit of a lifetime of research. The title is far too modest, for Abel Chatelain has produced not just the first history of temporary migration in France but a fundamental contribution to our understanding of

the global process of economic modernization and nation building.

We all know about the masons from the Creuse and perhaps the longsawyers from the Haute-Loire. But Chatelain takes us into the worlds of vinedressers and grape harvesters from the Cevennes and the Causses; the hordes of hay and grain harvesters from all poor regions; the lumberjacks, strippers, and charcoal burners from Auvergne; the complexly organized transhumant shepherds from the Dauphiné, Haut-Provence, and the Cevennes; the hemp-combers, sheepshearers, silk-gatherers, roofers, masons, shoe repairmen, tinkers, *muletiers*, carters, and peddlers from Auvergne and Limousin; the used goods dealers, the ragpickers, the chimneysweeps herded about by fathers and uncles, and the itinerant tutors *qua* servants from the Alps; the Auvergnat knife grinders and water carriers, the Savoyard shoeshine boys and porters made famous by Mercier; the building trades workers, common laborers, *terrassiers*, tradesmen, and domestic servants that flocked to nineteenth-century cities and industrial sites from various hinterlands; and the prostitutes and professional beggars from everywhere. This partial list suggests the range and complexity of Chatelain's field of study. A way of life for possibly a fifth of all French families toward the middle of the nineteenth century, temporary migration was without question one of the major formative influences in the history of modern France.

The bulk of the work is descriptive, making careful classifications of types of migrants and offering marvelous insights into the lives of this mass of people on the move. His central conclusions fall into three main categories. First is the general historical pattern that he has uncovered. Although temporary migration as a rural survival strategy stretches deep into French history, its greatest magnitude—here paralleling rural out-working—occurred with the advent of industrial capitalism. The first and greatest phase, peaking toward the mid-nineteenth century, was seasonal migration. This was unquestionably a major force in tying backward regions to the mainstream of French life as migrants' repatriated funds and their growing familiarity with the larger world undermined the isolation of their home areas from the nation as a whole. Chatelain's work serves as an important corrective to Eugen Weber's argument that trends toward an integrated France occurred only in the last decades of the nineteenth century (*Peasants into Frenchmen* [1976]). As the century wore on, labor demand in the receiving economies progressively broke the balanced work-year of seasonal migration and "pluriannual" migration (long-term stays in the host areas) expanded. While both forms remained strong down to 1914, a third trend, the

final step toward definitive migration, developed during the Third Republic: *migration viagère*, life-time migration with return "home" only at retirement. In the twentieth century a new phenomenon, daily migration, has appeared as bicycles, local public transport, and, finally, motorized private vehicles have allowed a minority of farm folk to take industrial and service occupations regionally available.

Second, Chatelain's analysis of the causes of temporary migration is comprehensive. The migrant sought cash returns for a variety of reasons: to add to his patrimony, to pay off heirs and provide dowries, to pay taxes, but above all simply to survive in areas traditionally hard-pressed to support the population by farming alone. Both the influx of cash and the outflow of hungry mouths helped to maintain the precarious balance of existence.

Third, the final one hundred pages deal with the broad consequences of temporary migration—largely for the sending societies. Demographically, temporary migration allowed for the maintenance of denser populations than otherwise would have been possible. Birth rates remained high with seasonal migration, though death rates often rose among the migrants themselves. Chatelain's conclusions regarding economic consequences contribute significantly to current controversies about the relationship between temporary migration and development. As in many sending societies today, migration succeeded in bringing cash back into the home economies, but productive use of it was minimal. Land values tended to become inflated and, in any case, land purchase was often made only as a future security or as a means of enhancing one's local prestige. Moreover, areas of intensive migration normally suffered declining agricultural efficiency. Chatelain's conclusion is unequivocal: while temporary migrants contributed mightily to the economic growth of those areas using their labor, their absence was clearly a factor in the systematic underdevelopment of their home bases.

Regarding family life, sexual morality, and religious practice, Chatelain's conclusions are vague and ill supported. The clergy generally took a dim view of the consequences of migration in all of these domains, but careful study on the local level will be necessary before a clear picture emerges. Finally, political consequences are only briefly analyzed. Again the reader comes away disappointed that Chatelain's encyclopedic knowledge of the overall process did not result in more penetrating analysis.

More generally, the work suffers from two broad analytical shortcomings. First, although statistics revealing short-term migration are notoriously incomplete and intermittent, certain demographic

techniques of a serial nature might have been employed to create a more solid picture of the historical transmutations of temporary migration. Second, despite the fact that he has produced the most comprehensive study of temporary migration ever undertaken, Chatelain does not attempt to relate his work to the categories of analysis or the theoretical battles that have grown out of the large literature on temporary migration in the contemporary world. Not only would he have thereby sharpened his general analytical framework, but he would have profoundly strengthened the impact of his conclusions.

Such criticism, however, should not unduly diminish the estimate. This book places the problem of temporary migration at the center of the study of economic development (and underdevelopment) in the modern world.

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DOMINIQUE VEILLON. *Le Franc-Tireur: Un journal clandestin, un mouvement de Résistance, 1940-1944*. Paris: Flammarion. 1977. Pp. 428.

Dominique Veillon's study of the French resistance movement and clandestine newspaper, *Le Franc-Tireur*, traces the development of the movement from the summer of 1940, emphasizing its primary interest in propaganda through the production of a regular monthly newspaper, tracts, and satirical pamphlets. The author outlines the gradual extension of the movement into a wider range of activities (formation of direct-action units, organization of *maquis*, and so forth) and describes its geographical expansion outward from Lyon, its point of origin and the area of its greatest influence. After providing a brief sociological description of the movement, based on a limited sample of resisters, Veillon offers a detailed examination of the content of the newspaper and a discussion of *Le Franc-Tireur's* role during the last two years of the occupation, within the framework of larger regional and national resistance organizations.

Based upon an examination of archival materials, published works, and personal interviews, the book contains no evident errors of fact, and the author is to be commended for the thoroughness of her research. Still, this study of *Le Franc-Tireur* is in some respects disappointing. The general reader will be overwhelmed by the elaborate cataloguing of names and places as the author's commentary passes from region to region. While a piecemeal, locality-by-locality approach to resistance activities accurately reflects the reality of clandestine conditions, such a presentation may obscure one's understanding of the general phenomenon. The

specialist will regret that the *sociologie* of *Le Franc-Tireur* cannot be considered representative of the movement as a whole. Although the author acknowledges that it is difficult to generalize from her statistics, she may not emphasize enough that her sample, three-quarters of which pertains to the leadership echelon (the *cadres*), reveals little information about the militants or sympathizers of the movement. Similarly, in an excellent synthesis and presentation of the major themes of the clandestine newspaper, Veillon suggests that the editorials and tracts offer the "best image of *Franc-Tireur*'s spirit" and reveal "le vrai visage" of the movement (pp. 261, 262). Other historians, as well as former resisters, have pointed out that the writers of underground newspapers were not always in perfect harmony with the militants, who were often much less concerned with politics than were their leaders.

Notwithstanding these reservations Dominique Veillon's book is a worthwhile addition to a growing series of studies concerning the organized resistance in France during the Second World War. Of the major organized movements only the *Front National* and *Libération* (-Sud) now lack satisfactory monographic treatment.

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MODESTO ULLOA. *La hacienda real de Castilla en el reinado de Felipe II*. 2d ed. (Publicaciones de la Fundación Universitaria Española. Monografías, number 19.) Madrid: Fundación Universitaria Española. 1977. Pp. 889.

European historians are rightfully sensitive to the question of the economic impact of the early modern state. Intervening in all sorts of fiscal and economic affairs, the state played a decisive role in the gradual transformation from feudalism to capitalism during the early modern period. Nowhere was this development more evident than in Spain, particularly during the reign of Philip II. The overwhelming financial requirements of the Habsburg war machine demanded a steady increase of fiscal exactions at all levels that produced severe pressures in every sector of the Spanish economy.

The problem, in historiographical terms, is not a new one, but a new book now exists that further clarifies the issues. At first glance, Modesto Ulloa's volume appears to be a republication of the same work that appeared in a limited edition in 1963. But the second edition is, in fact, a much different and a much larger book, encompassing the results of another decade of research. Moreover, this handsome volume has been produced in a university monograph series and will thus become available to the widest scholarly audience.

The importance of the work, in the first instance,

is a reflection of its sources. Ulloa has successfully and skillfully exploited nearly every fiscal section (as far as I can tell) of the vast Simancas archive relevant to Philip II. Consequently, his study serves as an introduction to portions of that repository, exploring both its possibilities and its limitations. And Ulloa remains very much aware of the extent to which fiscal data must be used with caution in constructing general hypotheses concerning economic developments during the period.

The study covers four areas. The first quarter of the volume concerns the structure of government, the costs of bureaucracy, and the fiscal situation upon the accession of the Prudent King. The author then details, in the course of more than four hundred pages, the tax structure of Castile as applied to individuals, regions, and various types of production and trade. A much briefer but invaluable section describing the fiscal relationships between church and state follows. The work concludes with a summary history of the fiscal situation from 1560 until the close of Philip's difficult reign.

Readers will be impressed by the abundant yet precise details on fiscal and economic matters. It is clear, for example, that the growing burden of taxes played an important role in depressing certain types of production, but the text highlights the extreme complexity of these relationships. For instance, wool production in Segovia was retarded by tax policies, yet the decline of woollens in Cuenca was more the result of the lack of raw materials, a factor related more directly to patterns of overland and overseas trade than to fiscal decisions. This balanced and sophisticated approach makes Ulloa's book a remarkable and necessary addition to the field. The work sets a standard that will make it difficult for scholars to generalize further about these questions without first considering the tremendous complexities that Ulloa analyzes in detail.

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ROLAND BAETENS. *De nazomer van Antwerpens welvaart: De diaspora en het handelshuis De Groote tijdens de eerste helft der 17de eeuw* [The Indian Summer of Antwerp's Prosperity: The Diaspora and the House of De Groote during the First Half of the Seventeenth Century]. In two volumes. (Historische Uitgaven Pro Civitate, Series in-8°, number 45.) Brussels: Gemeentekrediet van België. 1976. Pp. 399; 370. f 980.

Central to this study are the fortunes of the famous Antwerp merchant family of De Groote. Primarily on the basis of financial accounts, Roland Baetens tries to reconstruct the trading activity of this firm

against the broader background of the political, economic, and sociocultural situation of Antwerp in the seventeenth century. This analysis, part one of the study, is actually a synthesis of existing literature on this theme, supplemented with some personal positions of the author. Although the prominent De Groote firm was, in many ways, typical, the author rules out a comparative market study. Such a project, he considers, could not be based on one single example. Of no less importance in his view is the study of the people behind the financial accounts and the despatches, in particular the psychology of the individual entrepreneur. This perspective leads the author to write a descriptive account of the origins, composition, and fortunes of the generations that preceded the foremost representative of the house, Balthasar De Groote. Baetens describes Balthasar De Groote himself as a typical entrepreneur à la Schumpeter—an innovator and a risk-taking empire builder who occasionally overestimated his own strength.

Baetens describes the internal and external organization of the De Groote firm, followed by a short treatment of its bookkeeping. He also provides some global criteria for assessing the growth and size of the firm, its finances, investments, and profits. Finally, he evaluates the importance of the enterprise for the community. The last section of the study deals with the firm and the market. It is essentially an internal analysis because, as the author regretfully notes, the extremely fragmentary nature of the source material and the paucity of the literature make a comparison with the foreign market situation difficult.

I have intentionally limited my review to the second half of this study—the De Groote firm—in order to illustrate the principal and most original contribution of the author. The work is a conceptual whole in which Baetens analyzes the diverse aspects of the firm's commercial activity. Once again, research demonstrates that historical investigation of the principal Antwerp merchant families of this period can be profitable. I hope that this study will stimulate further initiatives in this area, especially since such studies should determine accurately how long and to what degree the sun shone in the Antwerp "Indian summer."

In conclusion, I would like to make an observation in the technical area. Both volumes of this study contain a great amount of source material. It is probable that the author intends to provide his readers with a ready-made work instrument, but its usefulness is uncertain. Much of the numerical data would certainly be easier to deal with if the possibilities of graphic representation had been employed more often and more systematically.

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G. GROENHUIS. *De predikanten: De sociale positie van de geformeerde predikanten in de Republiek der Verenigde Nederlanden voor ± 1700* [The Preachers: The Social Position of the Reformed Preachers in the Republic of the United Netherlands before ± 1700]. Summary in English. (Historische Studies, number 33.) Groningen: Wolters-Noordhoff, 1977. Pp. viii, 214. f 29.50.

G. Groenhuis writes not to propound a thesis but to refute the current judgment that the pastors of the seventeenth-century Dutch Reformed Church came from the lower classes of Dutch society and thus represented religious and social attitudes frequently in opposition to those of the more latitudinarian regent class. He demonstrates that the Reformed clergymen occupied a middle position—the third of six social strata—in Dutch society when judged by their family connections or by their income and property. Moreover, after the first decades of the century their educational attainments matched those of the highest social group. Furthermore, they usually dominated ecclesiastical politics, and, although the seventeenth-century Reformed church was not actually a state church, its relationship to government was very close. The charge made by some seventeenth-century patricians that the clergy were low-born demagogues Groenhuis judges to be greatly exaggerated and at best applicable to very few of the preachers. Some of the pastors, too, complained that they were meanly regarded by the wealthy and powerful in the republic, but Groenhuis correctly tempers these complaints by depicting the pastors' expansive self-image—that of prophets and shepherds of the "New Israel." Groenhuis concludes that the seventeenth-century Dutch Reformed clergymen were part of a middle, upwardly mobile, social and economic group and that they generally behaved accordingly.

Most of the evidence for Groenhuis's position comes in the last of his five chapters. Though not quantitatively exhaustive, he presents enough data from baptismal records, marriage records, contemporary pamphlets, and other ecclesiastical archives to convince his readers that the pastors did not come from or represent the republic's lower classes. The other chapters provide background and set up the sociological machinery for the study: one describes the relationship between church and state, one develops the "New Israel" concept and its implications, and another explores contemporary images of the pastors. Chapter two evaluates several descriptions of the republic's class structure and several techniques for determining the clergy's position in it before isolating six social strata in seventeenth-century Dutch society and adopting (primarily following Ronald

Freedman) four rank systems by which to evaluate the clergy.

Perhaps all of the background material and sociological scaffolding are appropriate for a doctoral dissertation, which Groenhuis's study originally was. In a published monograph, however, these matters usurp too much space. Perhaps Groenhuis's findings would have been more effectively presented as a journal article than as a book. That it was written as a dissertation may also account for its somewhat fragmented organization and for the author's apparent reluctance to draw any larger implications from his conclusion. Groenhuis, however, argues his conclusion convincingly, and although he is not the first to challenge the lower-class identification of the Dutch Reformed clergy, he may be the first to apply systematic sociological analysis to the problem.

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GEOFFREY PARKER. *The Dutch Revolt*. Ithaca, N.Y.: Cornell University Press. 1977. Pp. 327. \$17.50.

The Revolt of the Netherlands is one of the fundamental events of modern European history. Without it Protestantism would have found it far more difficult to survive. England's rise to power and empire, had it taken place at all, would have occurred in very different fashion; more likely England would have lost its independence and become a part of the Habsburg dominions. The fragmentation of Germany might possibly have been reversed, with fatal effects on France and Ottoman Turkey as well, while the New World, very likely, would be entirely Spanish-speaking as well as entirely Catholic. Thus, a history of the modern world with the Dutch left out is wholly meaningless. Far too many of us do leave them out or at best think of them as quaint growers of tulips or consumers of herring and gin.

Our ancestors knew better. It was in 1856 that John Lothrop Motley published his magisterial *Rise of the Dutch Republic*. For his day he was a scientific historian, and he had spent ten years on his project, five of them in the archives. His book and its sequels were widely read for many years, and they deserved it. If Motley did not write as well as Prescott or as Parkman, no one else did either; nor were other scholars prepared to follow Motley into his chosen archives. He had very little competition in his field for many years. Even after Pieter Geyl published his *Revolt of the Netherlands* (1931), the Dutch themselves continued to read Motley, whose volumes were still considered a standard work when I went to The Hague to study twenty years ago. In this country sets of Motley

were often seen on private bookshelves before the Second World War. He was still being read then, not only along the banks of the Hudson but in New England too. My generation grew up to believe that the Dutch contribution to the beginnings of America was an important one.

Today people do not have time to read all of Motley. If they did, his confessional bias would be almost intolerable, while his dislike for Philip II would seem ludicrous. A great deal more is known now than it was possible for Motley to work out a century and a quarter ago. Not too much of this new material is available in English. Geyl's work was not entirely sound nor was it without bias of its own. Thus, this new work of Geoffrey Parker ought to receive a warm welcome at several levels. It is an admirably well-ordered and well-written outline of the events of the years 1549-1609, presented in less than three hundred pages of text. For the general reader or for the undergraduate that may be enough. For the student, the bibliography and the footnotes have an independent value as guides to the rich literature available in English, French, German, Dutch, and Spanish. And for once a book in this field has adequate maps.

Parker, unlike his predecessors, knows his economic history. Indeed, one might wish that he had put more of it into his story than he did. The political and religious themes remain dominant, perhaps excessively so. In a book of this length some themes have to be chosen and others excluded pretty rigorously, to be sure. But did the book have to be quite this short? At the end there is rather a mad dash from 1609 on down to 1667 and an irrelevant quotation from Pepys, chosen because it contains a fashionable obscenity. It would have been more useful to take another two or three signatures to tell us how the story came out, and to expand a few sections that are at present overcompressed. Not every author, however, leaves the reader crying for more. Geoffrey Parker has rendered a real service to the study of history, and he has written so well that he deserves to have a wide popular success.

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W. M. CARLGREN. *Swedish Foreign Policy during the Second World War*. Translated by ARTHUR SPENCER. New York: St. Martin's Press. 1977. Pp. ix, 257. \$16.95.

The present work is a shortened version of W. M. Carlgren's massive and informative study, *Svensk utrikespolitik, 1939-1945*, published in 1973 and commissioned by the Swedish government. Carlgren,

who is a respected scholar and also the director of the Swedish Foreign Office Archives, finds little to criticize in Sweden's policy during these traumatic years, although he does not neglect his responsibilities to *Clio*. The present work lacks the full documentation of the original, but concerned scholars can always refer to the Swedish edition. Carlgren was the first to have full access to official documents and papers that are now available to younger doctoral candidates, who, in turn, have produced a number of studies of greater depth on specific topics, always supplementing and occasionally improving on Carlgren's own study.

The book opens with a brief analysis of the power balance in northern Europe and the Baltic both before and just after the outbreak of hostilities. Sweden's interwar disposition to work with and within the League of Nations had cooled by the mid-1930s; Sweden and some of its Nordic neighbors had begun to reclaim independence of action and to reassert neutrality as a guideline for policy. When war erupted the Swedes found themselves interpreting neutrality to mean those actions and policies that would keep the country out of war. Essentially, this meant only limited aid to Finland during the bitter Winter War, coolness toward Norway and Denmark when they suffered German occupation in the spring of 1940, and a calculated appeasement of Nazi Germany on such questions as the transit of troops and supplies through Sweden, first to northern Norway and eventually to Finland, when the latter country became a *cobelligerent* with Germany in June 1941.

These were not policies, Carlgren would have us believe, that reflected what the government preferred or what public sentiment would have chosen. They were hardheaded decisions of Per Albin Hansson's coalition government, based on the assumptions that any other policy could not substantially affect the course of the war and that direct Swedish participation could do no more than bring the misery and privations of war to an additional seven million people. It was a policy that was at once unheroic and unsatisfying; for some it was humiliating. It is quite true that Sweden knew it had to maneuver carefully between the major combatants in the first three years of the war. For both political and military reasons Sweden had to trade with Germany. But Sweden's need to retain similar contacts with the Western powers was also real. With regard to Soviet Russia Sweden felt compelled to tread carefully in order to retain some influence in Moscow on behalf of Finland. All in all, it was not an easy assignment.

Only in 1943 was there any perceptible change in Swedish policy. By then the government foresaw the likelihood of German defeat and gradually withdrew various concessions. Carlgren suggests

that there was always danger of German retaliation, but this theme has gained more credence than it deserves. Sweden's main concern was to avoid an overt pro-Allied stance that would leave Finland at the mercy of a Soviet offensive, possibly ending with the Red Army on the eastern shore of the Bothnian gulf. Hence the persistent, sometimes frantic, efforts of Swedish Foreign Minister Günther and his able Secretary-General Boheman to help Finland conclude a separate peace with the USSR. The task became easier only after the appearance of a Western, countervailing power on the European continent in June 1944.

As the war drew to a close, Stockholm tried to mediate a German surrender to the Western Allies, an effort rejected by Eisenhower as contrary to solemn agreements concluded earlier with the Soviet Union. The Swedes gave appropriate training to Danes and Norwegians who participated in the final pacification and reconstitution of their respective countries. They made substantial private and public gifts, and concluded generous trade agreements with Sweden's Scandinavian and Finnish neighbors. But these actions were not enough to spare the country from the ignominy of having been "neutral" while others "fought." Unhappily, few have stopped to consider that even a small power has the right, perhaps the obligation, to pursue its own self-interests.

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SEPPO ZETTERBERG. *Suomi ja Viro, 1917-1919: Poliittiset suhteet syksystä 1917 reunavaltionpolitiikan alkuun* [Finland and Estonia, 1917-19: Political Relations from the Autumn of 1917 to the Beginning of the Border State Policy]. (*Historiallisia Tutkimuksia*, number 102.) Summary in German. Helsinki: Suomen Historiallinen Seura. 1977. Pp. 303.

This book, a doctoral dissertation at Helsinki University, is a solid study of Finnish-Estonian political and diplomatic relations from the Bolshevik Revolution to the fall of 1919. As in the nineteenth century, the relationship between Finland and Estonia in the years 1917-19 was unequal. The more advanced and ethnically related northern neighbor had served as both a cultural and a political model for the Estonian intelligentsia; and as the two fledgling states emerged in the aftermath of the collapse of the Russian Empire, there continued to be much more interest in Finland among the Estonian political elite than vice versa.

Seppo Zetterberg emphasizes two specific aspects of Finnish-Estonian relations: the possibility of political union between the two countries and the question of Finnish aid to Estonia in the face of

the Bolshevik threat. On the Estonian side, the desire for a dual state (the most commonly mentioned historical models were Austria-Hungary and Sweden-Norway) reflected a consciousness of vulnerability to the designs of larger powers, but this option was only one of several—including a Baltic or Scandinavian alliance, a federated Russia, or even a British protectorate—being considered. In contrast, the Finnish leadership, with few exceptions, flatly rejected political union since it would risk Finland's own independence for the sake of a weaker neighbor. But the virtues of a non-Bolshevik Estonia were not lost on the White Finnish government that had just emerged from a bloody civil war in early 1918. Thus, mainly out of self-interested anti-Bolshevism, the Finns lent money and sent weapons and volunteer soldiers to help thwart the Soviet invasion of Estonia in the winter of 1918–19.

Stressing the role of *Realpolitik* in Finnish-Estonian relations, this study is a useful antidote to traditional nationalist interpretations, which tend to exaggerate the role of pan-Finnic sentiments in this period. Although it is Zetterberg's stated intention to offer equal coverage for both sides of the Gulf of Finland, this proves to be impossible because of source limitations. Although the author has made thorough use of Finnish archival materials, supplemented by various West European depositories, he has not had access to Soviet Estonian archives. Thus, there is a significant dichotomy in the depth of coverage as well as in the strength of documentation. Written in Finnish, the book is obviously intended for a limited audience; some aspects, however, especially those concerning relations with the major powers, are of broader significance. For example, the vagaries of British policy during the Russian Civil War are illuminated here in microcosm. Although a short German summary is included, a more substantial presentation of the argument in a major language, say in the form of an article, would be welcome. Above all, this book provides a corrective on the diplomacy of small nations, viewing them not merely as pawns of the great powers, but rather as actors, however limited their options, on the stage of history.

TOIVO RAUN
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HARTMUT KAEUBLE *et al.* *Probleme der Modernisierung in Deutschland: Sozialhistorische Studien zum 19. und 20. Jahrhundert.* (Schriften des Zentralinstituts für Sozialwissenschaftliche Forschung der Freien Universität Berlin, number 27.) Wiesbaden: Westdeutscher Verlag, 1978. Pp. 332. DM 39.

The authors of this volume belong to a research

group on "historical modernization" that was founded at the Free University of Berlin in 1975. They define the group's two goals in a brief programmatic preface: first, to test the applicability of modernization theory and models for the study of German social history; and, second, to use quantitative techniques to examine historical problems such as urbanization, social conflict, political participation, and social mobility.

The essays presented here all reflect a commitment to these goals, but they pursue them in very different ways. Horst Matzerath examines the development of two Rhenish cities, Rheydt and Rheindahlen, in order to discover why the former grew rapidly while its apparently quite similar neighbor did not. Matzerath finds the answer in a set of social and economic conditions that combined to make Rheydt a vigorous economic force and an active social center. Hermann-Josef Rupieper bases his article on evidence about those imprisoned during the revolution of 1848–49 in Saxony. These data enable him to draw the social profile of political activists in that state: craftsmen, apprentices, and workers, especially from large cities, whose social and economic discontents encouraged them to become involved in revolutionary politics.

In contrast to these two microstudies, the other authors present synthetic treatments of general problems and national trends. By studying the pattern of strikes and lockouts in Germany from 1864 to 1975, Heinrich Volkmann tries to establish whether or not industrial conflicts have been "modernized." After a careful consideration of the length, size, intensity, and frequency of labor disputes, he concludes that they have become better organized and more extensive, but also briefer and more "rational." In his essay on "Research about Political Participation," Peter Steinbach treats approaches to, and problems in, the historical study of electoral behavior. Steinbach provides a valuable survey of the relevant methodological literature and also some useful remarks on the history of imperial German elections. Finally, Hartmut Kaelble gives us another of his distinguished contributions to the history of social mobility in twentieth-century Germany. Kaelble examines data on education and career patterns in an effort to determine if "modernization" increases the opportunities for upward social movement. His tentative conclusion is that vertical mobility did increase in the course of the century, but that this increase has been fairly slow and is most apparent in the middle ranges of the social spectrum.

Since each of these essays is part of a larger research project, they contain much more material than can be adequately treated in a brief review. Nor can we go into the vexed question of "modern-

ization" and its usefulness as an organizing principle for historical research. For our purposes it must suffice to point out how the essays exemplify the strengths and weaknesses of their approach to social history. In their intense examinations of a limited body of data, Matzerath and Rupieper show us the great advantage of microanalysis. They provide a fine-grained picture of the nuances and specificity of human experience, but neither author is totally successful in his attempt to connect this picture with national trends and theoretical perspectives. Almost the opposite situation occurs in the essays by Steinbach, Volkmann, and Kaelble. These authors command the broad sweep of social development; they give us national patterns and conceptual insight. But from these heights we sometimes lose sight of those disorderly specifics on which grander schemes must be based and from which they must derive their meaning.

In sum, the essays in this collection are all worth reading, because they make some important contributions to our knowledge, because they raise some significant methodological issues, and—last but not least—because they illustrate both the problems and the potential of a theoretically informed study of social change.

JAMES J. SHEEHAN
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EDA SAGARRA *A Social History of Germany, 1648–1914*. New York: Holmes and Meier, 1977. Pp. 473. \$35.00.

Over the last decade, a burgeoning interest in social history has begun to intrude on the hegemony that politics, diplomacy, and ideas have traditionally exercised over the writing of German history. Eda Sagarra's *A Social History of Germany, 1648–1918* is the first attempt at such a synthesis in English. Her use of recent research is complemented by the color and texture of life conveyed in memoirs and literature.

Sagarra's organization of modern German social history is bipartite, with 1806 as the watershed. In that year the Holy Roman Empire expired, and Prussia's annihilating defeat at Jena spurred reformers to grapple with the problem of nationhood. The choice of an essentially political dividing line—indeed, beginning and end points as well—is somewhat paradoxical, and one wishes that Sagarra had dealt more systematically with her conception of social history. But whereas German social historians tend to be almost stereotypically insistent about theory, Sagarra, professor of German at Trinity College, Dublin, is nonchalant. Indeed, the book's lack of any introduction is disconcerting. Sagarra concludes that "economic and

technical changes, besides the growth and redistribution of the population in nineteenth century Germany . . . were the really significant factors in the evolution of her society up to . . . 1914" (p. 425). But the book's treatment of long-term forces is largely incidental.

On both sides of the 1806 divide Sagarra has chapters on court life, the nobility, the army, and the peasantry. As is appropriate to the greater articulation of society during the nineteenth century, two chapters on "town life" and "the educated classes" in part one expand into a whole section on "Bildung und Besitz" in part two, with separate chapters on "the bourgeoisie," "the bureaucrats," "education and élites," "industrial entrepreneurs," "the Jews," and "artisans and small traders." The industrial working force appears in part two, but there is also a necessary chapter on domestic servants. Somewhat anomalous is the first chapter on the Holy Roman Empire, whose reputation Sagarra tries to rehabilitate. Moreover, no parallel chapter elucidates the political structure of the German empire, although such a chapter might help illuminate the intersection between political and social history. German practitioners of social history have generally retained a strong interest in politics, as is perhaps understandable given the role of the German state. Sagarra has written something close to George Macaulay Trevelyan's "history of a people with the politics left out," despite a few allusions to political events. A possible factor in the absence of any extended discussion of the Second Reich is that part two, for reasons that are not explained, concentrates on the pre-1871 years.

Sagarra's book concludes with a portrayal of an inflexible yet anxious ruling elite, bolstered by assimilation of successful "new men" into the power structure, "choos[ing] war" as a way out of foreign and domestic dilemmas (p. 247). This conclusion follows well-trod (if partly controversial) paths but seems a bit too sketchy given the book's general neglect of politics; moreover, it is formulated somewhat confusingly. In ignoring politics and parties Sagarra oversimplifies the middle-class response to authoritarian government, which was not altogether acquiescent or prey to demagoguery.

Sagarra's social history is particularly commendable for its insistence that German and Prussian history are not identical and that Germany had many traditions. (For example, Bavaria's army and bureaucracy were largely bourgeois, unlike the Junker stereotype.) The book also contains two chapters on churches and religious life, and Sagarra criticizes social historians for ignoring this aspect of the life of the anonymous masses because of their own lack of interest. One wants to thank Sagarra for not forgetting women altogether, as

even the most advanced social historians typically do. But a final section on "The Other Half," besides the unfortunate title, seems an afterthought, placing women outside of any social context. It is anecdotal, heavily literary, and questionable in some of its assertions (for example, that interest in feminism was limited to north Germany, thereby ignoring Baden and Munich, in particular). Women occasionally appear in the text, but are largely exiled to the back of the book. Moreover, it is annoying to have universal and male suffrage confused.

Still, one can easily recommend Eda Sagarra's book as an accessible introduction to its subject. Lively in style and anecdote, it provides a wealth of sometimes fascinating detail on the diverse lives of Germans.

AMY HACKETT
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ROBERT KOLB. *Nikolaus von Amsdorf (1483-1565): Popular Polemics in the Preservation of Luther's Legacy.* (Bibliotheca Humanistica and Reformatorica, number 24.) Nieuwkoop: B. De Graaf. 1978. Pp. 296. f 1.90.

Robert Kolb's work is timely for four important reasons. It has appeared during the years in which a large part of Protestantism is celebrating the quadricentennials of the Formula of Concord (1577) and the Book of Concord (1580), and it contributes to the background history of both these Lutheran monuments. Second, an increasing amount of scholarly attention is now being directed toward the "late Reformation" and Nikolaus von Amsdorf, though born only twenty days after Luther, lived on nearly twenty years beyond his death and was involved in the doctrinal controversies and struggle with the Habsburgs that determined the course of the Reformation in the second half of the century. Third, Amsdorf was not a creative or original theologian, but he directed his constructive and polemical writings to the people, so that this study sheds indirect light on popular religion in the Empire. Fourth, Amsdorf, though a second-rank reformer, represented one major effort to remain true to Luther's teaching. If Mark Edwards has taught us about Luther and the false brethren, Robert Kolb has presented a masterful intellectual biography of one of Luther's most faithful friends.

Amsdorf once declared, "I have joined the Reformation not on account of my love for the man Martin Luther, but on account of my love for truth." He arrived in Wittenberg a year before Luther and in his *Dialogus*, a utopian description of Wittenberg in 1508, Andreas Meinhardi described

Amsdorf as a "thorough master of the liberal arts, devotee and keen follower of Scotism. Although he is of noble birth, yet he has not hesitated to study the noble arts." Amsdorf's place in history, however, was not to be won as a humanist or scholastic, but as the stalwart friend and champion of Luther's evangelical religion. He was with Luther at the Leipzig debate and at Worms, and, when Luther withdrew to the Wartburg, he assumed many of his preaching and teaching duties. In 1524 he became the key figure in winning Magdeburg for Lutheranism and he evangelized Goslar, Einbeck, and Meissen. In 1542 he became the bishop of the first Lutheran diocese at Naumburg-Zeitz. In 1552 he moved to Eisleben, where he remained until his death.

During the two decades after Luther's death he struggled to preserve Luther's doctrine in truth and purity against the Philippists with their position on adiaphora, submission to the Leipzig Interim, compromises in the Augsburg Interim, propapalism, and crypto-Calvinism. He was involved to the point of exhaustion (both his own and that of his opponents) in the Osiandrian, Majoristic, and synergistic controversies. The author does not spare him for his own deviation from Luther's doctrine—by taking extreme positions, teaching a double predestination, and declaring good works to be harmful to salvation.

This is an excellent study of the role played by "God's knight and Christ's exile" in the post-Luther and pre-Orthodoxy period. The author has a most impressive and precise knowledge of theology and church history. One might perhaps wish for a clearer portrait of Amsdorf as a human being. Though he writes with great skill, the author would do well to avoid modish words like "stance." If the awkward term Gnesio-Lutheran was not used until the eighteenth century, perhaps it could now be replaced with another. The publisher should learn that with the help of an IBM composer the margins of camera-ready type, too, can be justified.

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GERHARD BRENDLER and ADOLF LAUBE, editors. *Der deutsche Bauernkrieg, 1524/25: Geschichte, Traditionen, Lehren.* (Akademie der Wissenschaften der DDR. Schriften des Zentralinstituts für Geschichte, number 57.) Berlin: Akademie-Verlag. 1977. Pp. 473. 48 M.

The four hundred and fiftieth anniversary of the German Peasants' War of 1525 served as the occasion for an extraordinary outpouring of scholarly publications on that event, which points up the

renaissance of the study of the insurrection that has been going on for some time and behind which lie broader and less arbitrary causes than its anniversary. Although English-speaking scholars have made notable recent contributions to our understanding of the Peasants' War (for example, on this side of the Atlantic, David Sabean and Lawrence Buck), we have to look primarily to changing conditions in East and West Germany for an explanation of the volume and quality of Peasants' War scholarship.

In West Germany, perhaps largely because of political changes within and without the universities, a new generation of academics has been more receptive to different scholarly approaches to the Peasants' War. In East Germany, interest in the Peasants' War has long been a matter of course, and the book under review raises the question—what are the current interests and approaches of East German scholars?

The forty-one contributions that make up the volume comprise most of the papers presented at a conference in Erfurt in November 1974 under the auspices of the *Akademie der Wissenschaften* of the German Democratic Republic. Of the forty-two authors, the great majority are academics from the German Democratic Republic (excepting two each from the USSR and Bulgaria, one each from Czechoslovakia and the U.S.).

The topics may be summarized as follows: four essays place the insurrection into the context of developing capitalism and the "early bourgeois revolution" (Steinmetz, Čistozvonov, Laube, and Heitz); five deal with the ideas or practice of Thomas Müntzer (Smirin, Brendler, Kolesnyk, Gutsche, and Nikolov); four treat various aspects of the intellectual history of the event or of Germany about the time of the event (Looss on Catholic polemic, Schreyer-Kochman on a pamphlet by Hans Hergot, Entner on humanism, and Schildt on the German language); and five deal with various special topics on the Peasants' War (Hillerbrand on the Reformation and the Peasants' War, Mühlpfordt on burghers, Vogler on some aspects of the seizure of political authority by rebels, Schnitter on the programs for military organization, and E. Ullmann on the destruction of images and the plastic arts).

Other topics are more remote from the event itself: peasant insurrection outside of Germany, sometimes tied or compared to the German insurrection, sometimes not (Hroch on Bohemia, Hoffmann on Russia, Jonov on Bulgaria, Schilfert on England, Kossok on Spain, and Zeuske on Latin America); other German peasant movements (Wernicke, Harnisch, Reich, and Bleiber); the legacy of the Peasants' War in socialist practice, historiography, or art (Bartel, Schmidt, Noack,

H. Ullmann, Küttler, Berthold, Kinner, Hinckel, and Gärtner); and present peasant policy in East (Holzschuh/Heun, Schiller, and Graffunder) and West Germany (Zimmermann).

Thus, the range of topics is wide. Restricting my comments to those essays more closely connected to the Peasants' War and its background, the following characteristics of the essays seem noteworthy. First, essays on the ideas of rebels and their intellectual or cultural setting are numerous, as are essays sketching large subjects in short compass and based on secondary literature; absent altogether are archival studies of events or of the socioeconomic background to events within localities or regions (see, however, the essay by Reich on the eighteenth-century "Kohrener Landes"). I wonder how Karl Marx would have judged this choice of subjects by the forty-one scholars in officially Marxist states. Second, it would be difficult to demonstrate from this volume the recently noted tendency for increased scholarly dialogue between East and West on the subject of the German Peasants' War. Apart from primary sources and much older secondary literature, text and footnotes refer almost solely to material published in Eastern Europe and the Soviet Union (but see the suggestive article by Vogler).

JOHN C. STALNAKER
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HANS JOACHIM BERBIG. *Das Kaiserliche Hochstift Bamberg und das Heilige Römische Reich vom Westfälischen Frieden bis zur Säkularisation*. In two volumes. (Beiträge zur Geschichte der Reichskirche in der Neuzeit, numbers 5 and 6.) Wiesbaden: Franz Steiner Verlag, 1976. Pp. lvii, 202; vi, 203–508. DM 56 per volume.

Overshadowed by the titanic struggle of Prussia and Austria, the multitude of smaller German states has seldom received its due attention. Confronted with the study of such modest states, the historian has two options—to examine the particular history of an individual principality or to develop some type of comparative study. Berbig has chosen a middling-sized ecclesiastical state, Bamberg, and has opted for the second alternative. The diplomatic and political nature of the relationship of the prince-bishops of Bamberg to the emperor, the pope, and other states confines this monograph to certain limits. One searches in vain for analysis of the social, administrative, or economic characteristics of the state; Hans Joachim Berbig has enough political material to preoccupy himself and could not, or would not, develop the internal history of Bamberg. The chief virtue of his work lies in his exposition of the intricate maneuvers

whereby the prince-bishops endeavored to preserve the independence of the state. Bamberg was a smallish, if somewhat fat, fish in a German sea in which the Prussian, Austrian, Bavarian, and French sharks sought nourishment. It is a minor miracle that it survived until the Napoleonic era. Berbig's chief failure is indifference to the state itself, since he seldom reveals any information about how the prince-bishop ran his administration, what the inhabitants were doing, or how they lived; Bamberg remains, after two volumes, terra incognita.

Escaping from the bonds of the local estates, the prince-bishop became a kind of absolute monarch over a population of 197,000 after 1648. Aware that its small size made it perpetually weak, successive rulers tried to obtain union with neighbors and actually achieved it with Mainz for thirty-six years and Würzburg for sixty-three years. Such arrangements did not, of course, result in the creation of unified states, since local political interests remained entrenched. Although pope and emperor exercised control over the office of prince-bishop through election and confirmation, in actuality, certain prominent aristocratic families succeeded in dominating not only the episcopate but also the college of canons throughout the entire period. The popes were weak in Germany after 1648, although sometimes a particular nuncio tried to make the prince-bishops work together under his direction. The only result of such efforts was to drive Bamberg closer to antipapal Austria. Joseph II tried, with a diocesan regulation, to deprive some of these princes of their prerogatives and to subordinate them to an overriding Febronianism. Resisting this attack, the bishop of Bamberg developed his own Febronianism. Prussia became a real danger to such states under Frederick II. The immediate reaction of Bamberg was to consider that Prussia intended to secularize it or make it Protestant, or otherwise destroy its identity. The obvious policy followed by its rulers was to make closer alliances with the emperor. Enough has been said to indicate that Bamberg could make no lasting alliances with any power but had to pursue a policy of neutrality and temporary alignment with whatever state seemed to be the least dangerous at the time. Naturally, during the French revolutionary wars such a policy was completely inadequate. Despite strenuous efforts of the last bishops, Bamberg was finally secularized and incorporated into Bavaria in 1803.

Bamberg, as the author maintains, was similar to other states in *Germania Sacra* in basic diplomatic and political policies. It differed from many in its old-fashioned aristocratic mold, and, perhaps, in other internal respects (the author does not investigate these possibilities). Limited as this study is,

it possesses certain value in its exposition of the political life of a fairly representative German ecclesiastical principality. Those interested in the German policies of Prussia, Austria, and France during the period from 1648 to 1803 will find some minor insights; French General Augereau was deathly afraid of destroying these petty states in 1801, for example, because he thought that a general *levée en masse* of Germans would follow, which would be directed against France as well as Austria.

HUBERT C. JOHNSON
University of Saskatchewan

URSULA A. J. BECHER. *Politische Gesellschaft: Studien zur Genese bürgerlicher Öffentlichkeit in Deutschland*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 59.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 230. DM 52.

After the fall of the Holy Roman Empire in 1806, whatever Whig history there was in Germany concentrated on the smaller states. The dominant Borussican school of the Bismarck era emphasized a conservative historiography that viewed the development of parliamentary institutions and ideas of popular sovereignty as mere expressions of group self-interest, as archaic, as obstacles to the inevitable, progressive development of the national state produced under the "modern viewpoint of the aggressive royal leader." The revival of parliamentary government in the Bundesrepublik has recently made possible a new generation of Whig historians. Yet, most modern political historians still tend to be enmeshed in the presuppositions of the Borussican historiography. One therefore welcomes a new study of the Whig-oriented political thinkers, mostly *Reichspublizisten*, who appeared in eighteenth-century Germany, and who often attacked despotism and tyranny in no uncertain terms. Ursula A. J. Becher's study is one of the first modern efforts to find a balanced perspective on the origins of the latent German Whig tradition.

As the German political historians of the eighteenth century have done very little substantial archival research, the author is constrained to work with certain constructs, which social historians like Reinhart Koselleck and philosophers like Juergen Habermas have brought into the contemporary discussion of how modern society arose and how modern forms of representative government appeared. Instead of an interest in classical liberalism or Whiggery or representative government as such, there is thus a concern with "political society." The author is interested in whether the political writers of the late Enlightenment, whom

we may deem to be protoliberal, had fully developed ideas or foreshadowed ideas characteristic of a "political society." All that is really meant is liberal parliamentary government. The few writers who produced the works studied were all Protestants and belonged to a group who were often supported by the leading Protestant states of the Reichstag, that is, Hanover (Great Britain), Schleswig-Holstein (Denmark), and Prussia. The dearth of archival research makes any in-context study impossible. As one misses the background of politics in which these writers appeared, through no fault of the author, it might be useful to point out some of its salient features. Johann Jakob Moser and his son Friedrich Carl von Moser, as well as the Göttingen jurist Johann Stephan Pütter, were all actively involved in Reich diplomacy aimed at saving the Württemberg constitution from the encroachments of Duke Karl Eugen during and after the Seven Years' War. Once the war ended, the states that had guaranteed the Protestant establishment and the parliament (estates)—that is, Hanover, Denmark, and Prussia—carried on extensive diplomatic intervention in Vienna and Stuttgart. The case was settled in the Reich Supreme Court in favor of the estates in 1770. That both Mosers were writing extensively on constitutional questions and on the problems of representative government, as well as on individual rights, results from this crisis in Reich affairs and is colored by the fact that tact had to be used until the elder Moser was released from his five-year imprisonment (hardly touched upon in this book).

The analysis of the works of the two Mosers is indeed excellent, and despite the still prevailing reluctance in German academic circles to call despotism by its name, the author has asserted a new and independent view and promises to be an important historian of ideas. Both Mosers believed in the protection contained in the Reich constitution and developed not a theory of citizenship but a more moderate view of what constituted subject and lord. Even the princes were "subjects" of the Kaiser and Reich. The Kaiser did not have absolute power and was urged to play his role, to offer leadership in the Reichstag (an assembly of princes), to maintain justice as promised in the compacts, guarantees, and constitutions through its courts, and to maintain the local estates (which were being suppressed everywhere).

The later portions of the book deal with the impact of the French Revolution on contemporary writers, especially F. C. von Moser and A. L. Schlözer. Becher omits many other leading publicists of the revolutionary era, however, including Johann Georg Schlosser, Goethe's brother-in-law. Most of the writers eventually reacted against the Jacobinism of the revolution. Yet Moser resumed a

liberal stance by 1796 and Schlözer, whose *Staatsanzeigen* at first hailed the revolution and then evinced antipathy to Jacobinism, continued to hail the positive achievements of the Declaration of the Rights of Man. What German liberals gained from the experience of the French upheaval was a fear of revolution, a new regard for "nation" (now conceptualized as the prerequisite for the development of a "political society"), and a characteristic belief in reform that came to dominate the next era. On the whole this is one of the best modern analyses of German liberal thought in the eighteenth century, and one eagerly awaits the author's next book.

HELEN P. LIEBEL

University of Alberta

LIESELOTTE CLEMENS. *Old Lutheran Emigration from Pomerania to the U.S.A.: History and Motivation, 1839-1843*. Translated by JAMES LAMING. Hamburg: Pomeranian Foundation. 1976. Pp. 113.

What role do motives play in the processes of history? Are they only the conscious desires of the participants, or do they include men's subconscious responses to forces that they barely understand? And how do motives differ from causes? Such problems, which can occupy historians and philosophers through many a long winter evening, do not daunt Lieselotte Clemens. With remarkably little concern for the theoretical implications, she plunges into an examination of the motives of Old Lutheran emigration from Pomerania. Her basic narrative depends heavily upon Wilhelm Iwan's document-laden *Die altlutherische Auswanderung um die Mitte des 19. Jahrhunderts* (1943). Although there may be some benefit in making the story of Pomeranian emigration more readily available in English, Clemens adds little that is new.

In attacking the chosen problem of motivation, both the materials and the methods used by Clemens are simply inadequate to the task. The author proceeds mostly by exegesis of documents from Iwan and elsewhere, torturing statements found in them to disclose whether the emigrants acted out of "religious" or "worldly" concerns (all possible motivations seem to reduce to these two categories). These strained efforts sometimes verge upon the ludicrous. When, for example, an immigrant in Buffalo writes that his fellow Lutherans back in Pomerania are "suffering great hardships in body and soul," Clemens remarks that this implies both religious and material motives, "just as in the Bible the expression *body and soul* . . . includes both the physical and the spiritual man and forms a unit" (p. 89). Overall, Clemens comes to the not surprising conclusion that both religious and economic motives were at work.

The numerous illustrations in the volume, although having little direct relevance to the text, suggest that the author hoped to reach a more popular audience. But the style and format of the book detract from this purpose; it is unpleasant to read. Its organization is that of a rather pedestrian formal academic paper, arranged as an outline with elaborate and confusing rubrics, under which are grouped assemblages of notes and quotations. The translation by James Laming is somewhat lifeless and sprinkled with Britishisms that may seem strange to the American audience for whom the book is intended. Indeed, it is difficult to visualize the precise audience the publishers hoped to reach with this book, lacking as it does both popular appeal and the intellectual rigor that would attract scholarly readers.

JAMES M. BERGQUIST
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VOLKER DOTTERWEICH. *Heinrich von Sybel: Geschichtswissenschaft in politischer Absicht (1817–1861)*. (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 16.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 420. DM 92.

Following the long-established, but not always honored, model for dissertations in history accepted by German university faculties, Volker Dotterweich's study of part of the career of Heinrich von Sybel as historian, organizer of historical institutions, and politician is rigorously organized, based on extensive research in archives and existing literature, and so succinctly introduced as to summarize the work at the outset. Monographic rather than biographic in intent (Sybel's wife is mentioned in a single paragraph), the book is divided into four unequal parts, each of which focuses on a topic. Although this device allows Dotterweich to refer briefly to those subjects he judges to be sufficiently studied in already published works and then to elaborate on other points related more clearly to his thesis, it does not permit a precise parallel with the chronological course of Sybel's life.

For example, the first brief section brackets Sybel's family background and youthful experiences with his university studies and the outset of his academic career up to the call to Marburg in 1846. In it emerges Dotterweich's argument that Sybel's outlook on state, society, and politics reflected the triad of *Beamtentum*, *Bildung*, and *Besitz* that in turn had formed his Rhenish liberal origins. But in a long second section Dotterweich returns to 1841, when the faculty at Bonn accepted Sybel's *Habilitationschrift* and thus assured his academic independence, in order to develop the ties among his

historical production, political thought, and political action as far as the call to Munich in 1856. This section is itself subdivided into three distinct studies. The first concludes that Sybel perceived of history as a chain of related periods and epochs in which the present shaped the future. The second study takes up Sybel's search via the ideas of Edmund Burke for the optimal political system, a liberal-conservative synthesis of middle class and bureaucracy in a constitutional monarchy. The third study in this section combines Sybel's role in the turmoil of the 1848 revolution and his gradually growing option for a *Kleindeutschland* with his concurrent preparation of a history of the French Revolution in which he stressed economic and social factors.

Dotterweich devotes a slightly shorter third section to Sybel's role as organizer of historical institutions during his Munich professoriate. Opposed and feared as a Prussian Protestant, he maintained himself as head of the history seminar at the university, secretary of the Historical Commission, and founder of *Historische Zeitschrift* as long as he had the backing of King Max II, for whom he filled the roles of political and cultural advisor and salon guest. This section emphasizes Sybel's significance in championing "scholarly" history over parochial interests in both church and state as well as his political skill in achieving his goals.

A brief final section centers on the 1859–61 period, when the Italian war drew Sybel back into politics, and the continuing question of the organization of Germany and Prussia's role in that organization. The attack from the ultramontane, nativist press intensified, and, when Sybel failed in his attempt to use his call to Bonn to bargain for more support from the king, he decided to move back to the Rhine. Dotterweich interprets this move as a dividing line in Sybel's biography, for it began a decade when politics dominated scholarship.

Among the many conclusions Dotterweich draws from his study of Sybel's historical thought, three stand out as especially pertinent for the subsequent development of German ideas. One is that for Sybel the "nation" was a state-nation, developed historically out of a common political experience, rather than a genetically based *Volk*-nation. Second, Sybel perceived the centralization of power in the hands of the crown as a "progressive" development. And, third, in the end Sybel had to conclude that the final measure of the historical validity of a development was quite simply its success.

Dotterweich hopes to continue and complete his study in the not too distant future. His readers will hope he does.

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HEINRICH GRAETZ. *Tagebuch und Briefe*. Edited by REUVEN MICHAEL. (Schriftenreihe Wissenschaftlicher Abhandlungen des Leo Baeck Instituts, number 34.) Tübingen: J. C. B. Mohr, 1977. Pp. xiv, 469.

Although scholars universally acknowledge Heinrich Graetz (1817–91) as the foremost nineteenth-century historian of the Jews, they often overlook his impact as a Jewish leader. In addition to writing an eleven-volume history of the Jews and numerous monographs on Biblical and historical themes, Graetz participated actively in the major Jewish movements of his time. He achieved early fame as a champion of traditional Jews opposed to religious reforms. For nearly twenty years he edited the prestigious *Monatsschrift für Geschichte und Wissenschaft des Judentums*, thereby exerting enormous influence over contemporary Jewish scholars. During his forty years of teaching at the Jewish Theological Seminary of Breslau, the first modern rabbinical school and one whose curriculum he helped to shape, Graetz trained several generations of rabbis and scholars who went on to lead Jewish communities in a number of European countries and in the United States. The historian was also active in the *Alliance Israélite Universelle* and several proto-Zionist groups. Heinrich von Treitschke highlighted the importance of Graetz to his contemporaries when he scathingly denounced Graetz as the representative of proud and unsimilable Jews.

The diaries and correspondence of Graetz, recently published under the auspices of the Leo Baeck Institute, are rich sources for students of nineteenth-century history thanks to Graetz's diverse interests. The historian's correspondence with individuals throughout Europe contains echoes of numerous issues—both local and national—confronting Jewish communities in Germany, as well as abroad. The letters also bring to life an international coterie of Jewish scholars who shared manuscripts and information in an era when institutional Jewish archives did not yet exist. In addition, Graetz's letters to Moses Hess, Karl Marx, and leading Jewish scholars in Western and Eastern Europe add new dimensions to our knowledge of important nineteenth-century personalities. Finally, the frequent allusions to contemporary events scattered through these letters provide valuable insights into the political judgments of Jewish leaders. (Clearly, some German Jews were not nearly as myopic as present-day scholars would like to believe.)

In contrast to the wide-ranging concerns and self-confident tone of the letters, the diary is a moving document of despair and self-absorption written during young Graetz's *Wanderjahre*. Ensnared in an impoverished and insecure existence,

Graetz faced a bleak future because of his unwillingness to convert and his inability to function as a rabbi since he was an inept orator. Eventually, Graetz found himself in his battle against religious reformers, but not until he had broken with his idol and mentor, Samson Raphael Hirsch, a leading neo-Orthodox spokesman. Graetz's diary entries contain biting sketches of contemporary rabbis—especially of Hirsch and Zacharias Frankel—and valuable social data on the customs and mores of small-town Prussian Jews. (The manner in which Graetz courted his fiancée tells us much about the sexual and romantic attitudes of provincial German Jews.)

Reuven Michael, the editor of this volume, deserves commendation for bringing together the correspondence from scattered archives and for providing useful explanatory notes translating the Hebrew, Aramaic, Greek, Latin, and French phrases that pepper Graetz's prose. The notes also help the reader to keep track of dozens of individuals whose names appear in these pages. One regrets only that the editor omitted a subject index and an analytical introduction.

JACK WERTHEIMER
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FREDERICK GREGORY. *Scientific Materialism in Nineteenth Century Germany*. (Studies in the History of Modern Science, number 1.) Boston: D. Reidel, 1977. Pp. xxii, 279. Cloth \$28.00, paper \$13.50.

One of the intriguing questions in the history of science concerns the extent to which forces in the social and cultural environment impinge upon the development of scientific conceptualizations. The infamous German scientific materialists, who achieved notoriety in the 1850s both as scientific authors and as political radicals once associated with the abortive German revolutions of 1848–49, offer interesting possibilities of cultural interpenetration. The principals of Frederick Gregory's study—the well-known Karl Vogt, Ludwig Büchner, and Jacob Moleschott and a minor materialist, Heinrich Czolbe—were all trained scientists or medical men with a deep commitment to philosophy.

Gregory makes it clear that what we are dealing with in scientific materialism is not the materialism of scientists but the science espoused by materialist metaphysicians. No one in the group qualified as a first-rank scientist, nor was science even a primary motivation. Rather, the scientific materialists wielded scientific theories and data as ideological weapons in their widely heralded crusade against theology, political conservatism, and all arbitrary authority. Their avowed materialism was more a banner than an accurate description of

philosophical positions. Deriving their philosophy from the anti-Hegelian naturalism of Ludwig Feuerbach, whose thought Gregory reviews at length, the scientific materialists—for all their deterministic, reductionist rhetoric and their denials of free will—wrote works of intense moral fervor predicated on the perfectibility of man in nature. Büchner's *Kraft und Stoff*, Moleschott's *Kreislauf des Lebens*, and other controversial materialist tracts bore the mark of Feuerbach's revisionist idealism.

Gregory's themes are not surprising or new. Most German scientists of the period recognized their overly ambitious materialist colleagues as secret metaphysicians and ideologues rather than serious biological investigators. Yet, in the philosophy-laden atmosphere of nineteenth-century Germany, science was identified in many quarters with materialism. Germans affixed the materialist label promiscuously to all manner of political, intellectual, and cultural traditions. Gregory, therefore, takes special pains to give precise definition to his particular strain of scientific materialism and to differentiate it from science proper. He devotes a crucial chapter to issues in contemporary biology, including vital force, spontaneous generation, and evolution. His attempts at clarification only seem to muddy the relationship between materialism and German biology. Gregory depicts, for instance, the vital force espoused by Liebig at one point as a "naturalistic" agent subject to scientific laws and elsewhere as a metaphysical preference. Unfortunately, adding to the confusion is Gregory's muddled organization and an imprecise use of language, highlighted by some wildly mixed metaphors, such as "the fledgling eye of the budding Hegelian Karl Rosenkranz" (p. 18).

Gregory side-steps important comparisons by ruling out certain relevant traditions a priori. He will not consider, for example, Haeckel's monistic materialism, because, among other reasons, it belongs to a later period, and its romantic overtones were foreign to the materialists of the 1850s. This omission belies Gregory's intention to write a comprehensive study of German materialism in the second half of the nineteenth century and contradicts Büchner's leanings toward romanticism and monism. Gregory likewise dismisses from serious consideration the Berlin biophysical school of the 1840s, which others frequently brand as materialistic.

Gregory has shielded himself from other complexities by relying only on published pronouncements of his characters. He claims immersion in primary materials, but the photocopies of German script embellishing his text are thoroughly misleading; he uses hardly any of the rich documentary sources in German archives. The result is an exercise in philosophical and psychological ar-

gumentation rather than a full-textured history. Until the primary sources are fully exploited, Gregory's judgments about this controversial and puzzling movement must remain provisional.

ARTHUR P. MOLELLA
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HANNAH S. DECKER. *Freud in Germany: Revolution and Reaction in Science, 1893-1907*. (Psychological Issues, monograph 41.) New York: International Universities Press. 1977. Pp. xi, 360.

With *Freud in Germany* Hannah S. Decker has fulfilled the promise of her dissertation and several previous articles. This work illuminates the history of medicine, psychiatry, and psychology as well as that of psychoanalysis. It reveals the author's thorough grasp of historiographical considerations in addition to scientific developments.

Decker has carried out an exhaustive examination of the medical, psychological, and general literature of Wilhelmine Germany, including periodicals, conference proceedings, monographs, textbooks, and semipopular works. Her conclusions destroy the myth, created initially by Freud himself and propagated by later psychoanalysts, that Freud was either ignored or reviled in the years when he was laying the foundations of psychoanalytic theory.

Decker convincingly demonstrates several important points. First, Freud's early work did receive considerable attention, particularly from physicians and the lay public. Second, much of this attention was favorable or neutral, and even critical notices were usually polite and accepted Freud as a serious worker. And third, rejection of psychoanalysis was not due solely to anti-Semitism, sexual hypocrisy, or the critics' personal psychic resistances; hostility toward Freud also resulted from the historical development and theoretical concerns of German medicine and experimental psychology. Late nineteenth-century German scientists exalted positivist methods and materialist explanations and repudiated anything, such as the use of hypnosis or interest in unconscious mental phenomena, that smacked of unscientific Romantic *Naturphilosophie*. Hypnosis was also associated with French neurologists such as Charcot and Janet; nationalist sentiment was a factor in the reception of psychoanalysis, as in so much else during this time.

Yet the lay public was often enthusiastic about psychoanalysis, especially *The Interpretation of Dreams*. Many physicians expressed real interest in Freudian explanations of hysteria and dream interpretation. Decker makes it clear, however, that Freud made few converts in Germany before 1907

and that his audience was not large at any time before 1914. Indeed, she argues, his explicit monism, treating mind and body as a unified system and denying any sharp distinction between psychic sickness and health, contradicted the prevailing dualism of the science of his time. The intellectual revolution necessary to the widespread acceptance of psychoanalysis occurred only after the Great War.

Decker defines the premises and sets the limits of her inquiry with great care and conscientiously relates her subject to its general historical context. She expresses her theses precisely and develops them clearly; she defines technical terms and uses them accurately. The book is quite readable, though a bit repetitious on occasion, and contains an adequate index. The author uses the scientific style of documentation, which is sometimes scanty by historians' standards. A separate listing of periodicals would have been a useful addition to the bibliography. The publisher is at fault for a binding from which several pages have already come unglued.

But these are minor flaws. This is an excellent work, firmly grounded in thorough research, well written, and an important reminder that historical causation is rarely simple. The reaction to psychoanalysis was indeed "overdetermined."

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STERLING FISHMAN. *The Struggle for German Youth: The Search for Educational Reform in Imperial Germany*. New York: Revisionist Press, 1976. Pp. 150. \$49.95.

This essentially biographical approach to the problem of school reform in imperial Germany combines Sterling Fishman's previously published articles on the subject: an essay on Alfred Lichtwark, which appeared in the *History of Education Quarterly* (1966); the useful and suggestive essay entitled "Suicide, Sex, and the Discovery of the German Adolescent," which first appeared in the same journal (1970); and a chapter on Hermann Lietz from the *Paedagogica Historica* (1968). Fishman's attention to these interesting topics would have been more rewarding if he had developed a greater depth of analysis and a comparative approach, rather than merely offering his previous studies as they appeared originally. (He does not cite the original publication data anywhere in this volume.) The brief introduction and conclusion are new.

Fishman views the "three most important movements" of German imperial school reform—the art education movement, the country boarding school movement, and the naturalistic *Wanderzögel* move-

ment (p. 1)—as efforts to save Germany from "the pitfalls [of] modernism and rapid industrialization" (p. 138). One can trace the desire for a German moral rejuvenation back to Fichte, Father Jahn, and even to Luther; this desire is indeed a significant aspect of German history, especially under the strains of late nineteenth-century political and economic aspirations and frustrations. If Fishman had developed the interrelationship between higher school reform and social problems in Germany to a greater degree, then the re-publication of these articles certainly would have been justified.

Instead, this study rests on anecdotal biographical details and summations of essays by the reformers. It is not enough to tell just this side of the story. The excellent monograph by Folkert Meyer, *Schule der Untertanen: Lehrer und Politik in Preussen, 1848-1900* (1976) and other recent studies by Manfred Heinemann, Peter Lundgreen, and Wilhelm Roessler, although concerned with slightly different aspects and periods of education in Germany, all afford excellent methodological approaches to German educational history. It is disappointing that Fishman has not followed their tack.

Given the finality of some of the author's pronouncements, the documentation is often inadequate (on women in higher education, for example [p. 34]). From the sources cited—all published—it appears that Ludwig Gurlitt, Hermann Lietz, and Alfred Lichtwark received extensive attention in both the German scholarly and popular press. Fishman pays scant attention to other significant contemporary educational reformers, such as Georg Kerchensteiner, whose role merits adequate investigation. The author also does not demonstrate the interconnection between the reformers he does note, aside from pointing out that their ideas were similar. The most balanced chapter, concerning Lietz and "The Practice and Theory of Country Boarding School Life" (pp. 56-68), demonstrates the author's sensitivity to the daily conduct of the schools and the sacrifices they required from their idealistic founders, but similar description and analysis are lacking or very thinly scattered elsewhere in the study. Too frequent are the mixed metaphors, as well as pompous language and faulty spelling and/or typographical errors in both English and in German. This photoduplication of a typescript is not in any sense "revisionist," as the publisher's name implies.

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WOLFGANG STRIBNY. *Bismarck und die deutsche Politik nach seiner Entlassung (1890-1898)*. (Sammlung

Schöningh zur Geschichte und Gegenwart.) Paderborn: Ferdinand Schöningh. 1977. Pp. 359. DM 38.

That Otto von Bismarck, after his fall from power in 1890, criticized his successors and, indirectly, Kaiser William II mercilessly and implacably up to the last year of his life has always embarrassed historians. Motivations of petty spite and revenge did not fit the image of the great man. Consequently there has never been a serious, full-scale examination of the phenomenon. Wolfgang Stribny now fills that gap with a detailed study of Bismarck's activity and reactions to that activity.

Stribny finds that the old man's motives were patriotic rather than petty: Bismarck warned against moving away from Russia and relying solely on Austria-Hungary in foreign affairs, advice that neither administration nor public were prepared to heed; he denounced bureaucratic absolutism and advocated strengthening the Reichstag against the crown in domestic affairs. Stribny attributes Bismarck's failure to stress the latter point consistently after 1892 and his reversion in 1893-94 to a right-wing crisis policy aimed against the Reichstag to a basic conservatism. It is more likely that the motives behind Bismarck's actions were tactical ones. An economic downturn brought the full force of Caprivi's reduction of the grain tariff to bear upon the agricultural interests of the Junker ruling class, and Bismarck took advantage of the situation to encourage its rage against government and crown. Although the Right was in the minority in the Reichstag, it was still in control in the Prussian Landtag, army, bureaucracy, and court.

William II himself was probably the target of Bismarck's attacks in these years—through his ministers, his policies, the court, and army intrigues. Stribny barely mentions the Leckert-Lützow-Tausch cases of 1896-97, which revealed that the Prussian political police agitated in favor of Bismarck and against the government. It was in order to discredit the Kaiser and place him under wraps that Bismarck worked consistently to create a political crisis.

Stribny feels that the old man would have done better to try, as elder statesman, to educate the public peacefully to develop a more active and responsible attitude toward politics. But such an educational program—of doubtful impact at best—would be a long-term affair, and Bismarck, already seventy-five in 1890, had little time left. The personal instability of William II, the head of the authoritarian German power structure, was a present threat that Bismarck attempted to meet. He failed in this attempt, however, as he had failed previously to maintain himself in office, although Stribny correctly points out that Bismarck per-

manently dampened the Kaiser's popularity. The German middle classes that comprised Bismarck's loyal following were not capable of rebelling against authority; they merely wanted to be able to venerate all of their leaders equally and undisturbed. Nor were the Junkers, bureaucrats, and professors, who rallied most strongly to Bismarck and against the Kaiser's government, really capable of seizing the initiative or providing responsible leadership. Thus his campaign was probably doomed from the start.

It was brave of the old man to take on everybody in his old age, but it was also willful. In this respect Bismarck was not unlike the elderly Gladstone, who split the Liberal Party over the Irish problem. Although Bismarck's encouragement did not provoke the Junkers to seize political initiative from the crown, it did help to make them, in later years, more intransigent, self-centered, and less manageable by the government in a progressive sense. Mid-Victorian heroes accomplished brilliant things but also did a lot of damage one way or another.

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ALAN PALMER. *The Kaiser: Warlord of the Second Reich*. New York: Charles Scribner's Sons. 1978. Pp. 276. \$14.95.

Although Alan Palmer's brief biography of William II is obviously addressed to the general reader, it is more scholarly than most books of this kind and is based almost entirely on collections of contemporary documents, published and unpublished. Unlike many popular biographers, Palmer has not indulged in succulent speculations about the emperor's psyche or sex life, and with commendable restraint he has adhered to the most reliable or at any rate the most plausible evidence available to him.

Palmer thinks that William himself, as well as many later commentators, may have exaggerated the significance of his withered left arm, although he concedes that "the physical and psychological effects of this crippled arm undoubtedly left their mark on his character" (p. 3). He quotes William's tutor, Georg Hinzpeter, who recalled that what particularly impressed him "in this good-looking but girlish boy, whose delicate softness was turned into almost complete frailty by the embarrassing uselessness of his left arm, was his resistance . . . to every attempt which would have forced his inner self in one direction or the other" (p. 12), a round-about way of saying that William never learned the art of self-discipline. To the disappointment of his parents, "he became—and remained—an

arch-dabbler, a dilettante who showed promise but never fulfillment" (p. 13). Palmer is probably correct in assuming (since there is no reliable evidence to the contrary) that William was neither promiscuous nor homosexual, but he is certainly wrong in suggesting that the emperor was something of a prude. In his table talk and comments on official documents, William used the crudest kind of barrack-room language, and the fact that he attempted to impose a puritanical moral code on men under his command was surely only another example of his hypocrisy and delight in bullying others.

Unfortunately, in his admirable attempt to be brief, Palmer's coverage of many events of William's reign tends to be somewhat superficial. I was disappointed by his analysis of the English alliance negotiations, his inadequate linkage of the Moroccan Crisis of 1905 and the bid for a new alliance with Russia, and his apparent failure to see the ramifications of Witte's visitation after Russia's defeat by Japan. I also thought Palmer was far too kind to Eulenburg and that he did not give sufficient credit to Harden and other opponents of William's "personal regime."

Particularly questionable, in view of the evidence Palmer himself presents, is the subtitle of his book: *Warlord of the Second Reich*. His chapter dealing with William's role in the First World War is entitled "War Lord on Sufferance," a far more accurate designation. Moreover throughout his reign, whenever there seemed to be a serious threat of war, this saber-rattling exponent of German militarism displayed a most unwarlordlike loss of nerve. During the first Moroccan crisis, for example, when France's major ally, Russia, was immobilized by defeat in Asia (the most favorable opportunity for Germany to launch its "grab for world power," to use Fritz Fischer's phrase), William accepted a major diplomatic defeat rather than risk a foreign war. Although his army was far and away the most powerful in Europe, the emperor feared his forces were not adequately prepared and in any case could not be sent out of the country so long as there was a socialist menace at home. "Shoot down the socialists first," said this model monarch, who in 1890 had aspired to go down in history as the Labor Kaiser. "Behead them, put them out of action, if necessary massacre the lot—and then war abroad! But not before" (p. 116).

Palmer concludes his book by quoting Edward VII, who described his nephew as "the most brilliant failure in history." William II was indeed a failure, but brilliant?—only if brilliance be defined as an almost unerring instinct for saying or doing the wrong thing.

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HELMUT HIRSCH, *Der "Fabier" Eduard Bernstein: Zur Entwicklungsgeschichte des evolutionären Sozialismus*. (Internationale Bibliothek, number 104.) Berlin: Verlag J. H. W. Dietz Nachf. 1977. Pp. 159.

Eduard Bernstein (1850–1932) was a German Social Democratic writer and parliamentarian who is best remembered as the first "revisionist." Helmut Hirsch's new book and a companion volume by Thomas Meyer, *Bernsteins konstruktiver Sozialismus*, are signs of renewed interest in Bernstein's work in West Germany. Participants at a recent West German conference on "The Historical Achievement and Contemporary Significance of Eduard Bernstein" spoke of a "Bernstein Renaissance."

Hirsch's contribution to the "Bernstein Renaissance" is a short book (25,000 words plus appendixes) about Bernstein's contacts with members of the Fabian Society. Bernstein met many of the leading Fabians during his London exile (1888 to 1901), and he corresponded with several of the Fabians after his return to Germany in 1901. Hirsch was careful and thorough in his research. During his own exile from Germany, Hirsch earned a Ph.D. in history from the University of Chicago, so he is at home with the many English-language sources that he used for his study. He consulted manuscript collections in several countries as well as the published sources.

The broader question to which Hirsch addresses himself is whether the Fabians caused Bernstein to become a revisionist. Bernstein did write his initial critiques of Marxism while he was living in London, and he developed a respect for the Webbs and other Fabians at that time. Many of Bernstein's contemporaries and some of his biographers (for example, Pierre Angel and Bo Gustafsson) have concluded that Bernstein's Fabian acquaintances converted him to revisionism. Bernstein himself acknowledged that he had learned a great deal from the Fabians, but he denied that he had in any sense been converted to revisionism by them.

Hirsch concludes that Bernstein did in fact become a Fabian while in London, thus attributing more influence to the Fabians than do the authors of the most relevant English-language studies, Peter Gay and James W. Hulse. Both Gay and Hulse find ample evidence that Bernstein was influenced by his experiences and his observations in England, but Hulse comments in *Revolutionists in London* (1970) that "Fabianism was only one segment of the English scene, and not the one which [Bernstein] admired most" (p. 139). Hirsch provides some new evidence for future biographers, but his work, taken by itself, is inconclusive, partly because of a division of labor between himself and Thomas Meyer. Hirsch was to trace the personal contacts between Bernstein and the Fabians, while Meyer would analyze Bernstein's writings for text-

ual evidence of Fabian influence. Only an analysis of Bernstein's writings might disprove Bernstein's assertion, cited by Gay in *The Dilemma of Democratic Socialism* (1962), that the theories of the Fabians "had little to do with the specific questions raised by Revisionism" (p. 108).

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PETER WINZEN. *Bülow's Weltmachtkonzept: Untersuchungen zur Frühphase seiner Aussenpolitik, 1897-1901*. (Schriften des Bundesarchivs, number 22.) Boppard am Rhein: Harald Boldt Verlag. 1977. Pp. 462.

Using extensive unpublished documents and *Nachlässe* Peter Winzen offers here a very well-researched and important monograph on turn-of-the-century German foreign policy. The author rejects the heretofore accepted views that there was no single will controlling German foreign affairs and that the directing head was the shadowy Baron Friedrich von Holstein or Kaiser Wilhelm himself. He rejects, too, the simplistic characterization of Prince Bernhard von Bülow as merely an unbounded opportunist and an ambitious courtier. Instead, Winzen demonstrates that Bülow developed his own foreign policy based on his conception of Germany as a world power, and that the Kaiser accepted this policy. The conception had three components: national unity, fleet construction, and a Russophile diplomatic orientation.

Heavily influenced by Heinrich von Treitschke, Bülow proved a strong nationalist who rejected the dominance of one political, economic, or confessional belief over other considerations. He avoided all extremes and sought, after he became chancellor in 1900, to obtain the support of all forces in Germany that were willing to uphold the state. Once secured against domestic social revolution, Germany could then embark on a great policy abroad.

For Bülow foreign policy held primacy over domestic policy, but he made it dependent upon German naval construction. He asserted that until it had built a large battle fleet, Germany could not acquire a truly great colonial empire. In the interim he would pursue a policy of the "free hand"—defined as diplomatic abstinence—in order to steer Germany through the "danger zone." In particular, Bülow intended to avoid an irrevocable commitment to either of the two major powers, Britain and Russia. Of these two powers, nevertheless, Bülow consistently favored the Russians; he believed that the tsar's strong feelings for monarchical solidarity would in the end break down the Franco-Russian alliance and lead Russia

back into the German fold. But until the time was right the re-establishment of the old *Dreikaiserbund* would have to wait. In the meantime, Bülow tried to smooth over German-British relations. In this regard Winzen analyzes such issues as Samoa, the Boer War, and the developing problem of Morocco. While someone like Baron Hermann von Eckhardstein, first secretary of the German embassy in London, eagerly promoted a German-British defensive alliance, the chancellor dragged his feet on such a step and was perfectly satisfied when negotiations faltered in July 1901.

As the book points out, German difficulties stemmed from Bülow's superficial thinking. Subsequently, he would be unable to deal with unexpected changes in the European power structure. He remained unconcerned when the British abandoned their "splendid isolation" by signing an alliance with Japan in 1902. He was caught off guard by the emerging Anglo-French entente in 1904 and then totally miscalculated English-Russian relations. Bülow consistently counted on a British-Russian conflict coming sooner or later, for he never believed that they would resolve their differences in the Near and Far East. In such a war Germany would join the Russians, Britain would lose most of its colonies to Germany, and Bülow would then be assured a place in history side by side with Bismarck. When in 1907 the English and Russians settled their disputes, Bülow's design for world power ended in fiasco.

Winzen's painstaking examination unquestionably sheds new light on a German statesman hitherto imperfectly understood. The book is, therefore, a welcome addition to existing literature on early twentieth-century German diplomacy.

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KEITH BULLIVANT, editor. *Culture and Society in the Weimar Republic*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. vi, 205. \$18.00.

This volume of eleven essays, dedicated to Richard Hinton Thomas, is, according to the editor, not meant to offer a survey of Weimar culture but rather "to complement" the general studies of Peter Gay and Walter Laqueur. The spectrum of topics is certainly broad enough to appear to warrant the editor's claim. Some of the articles are of considerable interest; some break new ground. The editor's own contribution is an important discussion of Thomas Mann's attitude toward the politics of Weimar. Bullivant argues that Mann did not, contrary to the usual view, experience any kind of political conversion after the First World War but continued to view politics, and hence the

republic, in his traditional, esthetic terms; in other words, his support for the republic was, as his son Golo Mann put it, "literature, not reality." Rowland Cotterill argues with dexterity that *Mahagonny* is an underrated work and occupies a crucial transitional function in the development of opera. Godfrey Carr has seen fit to rescue from obscurity Friedrich Sieburg, the once widely read cultural critic and long-time foreign correspondent of the *Frankfurter Zeitung*. Ronald Taylor contributes an elegant and evocative note on *Wozzeck* and *The Threepenny Opera*. And Roy Pascal, using Adorno's critique of the traditional novel as his point of departure, has some characteristically provocative remarks to make about *The Magic Mountain* and Adorno. These are all worthwhile contributions.

For the historian of Weimar, however, potentially the most interesting essays are those by Hugh Ridley on the intellectual repercussions of Sergej Tretjakov's visit to Berlin in 1931, by Jost Hermand on *Neue Sachlichkeit*, and by Rob Burns on working-class literature. While all three articles contain noteworthy material, they also display most prominently the major weakness of this volume as a whole—its polemical inclination, implicit in most of the essays, overt in these three. His ideological commitment leads Hermand, for example, into an attempt to reorganize the nomenclature of Weimar culture. He contends that what has normally been labelled *Neue Sachlichkeit* should in fact not have that name because it was neither *neu* nor *sachlich*. Only the revolutionary wing of the expressionists actually deserved the name, we are told, no one else. And Rob Burns goes a step further in manifesting his political exuberance; he fabricates events to support his argument. An author whose stated aim is to "locate" working-class culture within its specific "historical context" would do well not to assert that there was a general strike in Germany in 1923 when in fact there was no such event.

At its best this volume presents some stimulating examples of current radical literary criticism; at its worst it degenerates into pure *Tendenzgeschichte*. Its unity is in its approach, not in its achievement. To say, then, as Bullivant does in his introduction, that the volume complements other more general studies is to understate completely the purpose of this collection. This book aims at nothing less than a radical reinterpretation of Weimar culture.

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DAVID KAHN. *Hitler's Spies: German Military Intelligence in World War II*. New York: Macmillan. 1978. Pp. xiii, 671. \$16.95.

F. W. WINTERBOTHAM. *The Nazi Connection*. New York: Harper and Row. 1978. Pp. 222. \$8.95.

David Kahn is no stranger to the subject of military intelligence, having published his popular *The Codebreakers: The Story of Secret Writing* in 1967. The present volume is equally ambitious—a comprehensive treatment of the total intelligence effort of one of the major protagonists in World War II.

The result is not without flaw. Its organization is disjointed, the content often repetitious, and the prose occasionally awkward (and was it really necessary to inflict "message" upon us as a verb?). *Hitler's Spies* would have benefited from more careful editing and closer attention to technical accuracy. It seems needlessly imprecise, for example, to declare one inch the equivalent of two centimeters. But these are relatively minor shortcomings. Drawing upon the documentary resources of the U.S. National Archives, the West German Bundesarchiv and Militärarchiv, as well as other depositories, Kahn has produced a book of considerable value to students of World War II and Nazi Germany.

Hitler's Spies illustrates the internal rivalry, inefficiency, and administrative confusion characteristic of the Third Reich. Four different groups shared the military intelligence function—the Wehrmacht, the Nazi Party, various ministerial bureaucracies, and the private sector, each contributing several intelligence agencies and all functioning without effective coordination. Some degree of centralization was the result of growing party influence in military affairs, notably the absorption of most military espionage responsibilities by the SS *Reichssicherheitshauptamt*, but this did not improve the quality of German intelligence, which, Kahn convincingly argues, failed at every strategic turning point in the war. Reflective of the sometimes ludicrous ineffectiveness of German espionage is the fact that virtually the whole of the wartime German spy network in Great Britain fell under the control of the British Secret Service, a circumstance described earlier in J. C. Masterman's *The Double Cross System* (1972). But Kahn's book is not limited to an examination of classical espionage activities, and in this sense the title *Hitler's Spies* is a bit misleading. Overviews of German aerial and ground reconnaissance techniques and operations, evaluation of captured equipment, and other more mundane aspects of military intelligence make useful and interesting reading.

Why did the German intelligence effort fall qualitatively short in comparison to that of the Allies? Nazi neofeudalism and its emphasis on will and charismatic authority were organizationally as well as spiritually antithetical to effective intelligence work, while the German conviction of

Germany's superiority to any enemy made it seem relatively unimportant. Moreover, Kahn argues, German commitment to the offensive aspects of war as opposed to the defensive ones (to which knowledge of the enemy's intentions is arguably more critical) encouraged neglect of the intelligence function. Although not entirely convincing, Kahn's suggestions are intriguing.

A less significant contribution to the historiography of World War II is F. W. Winterbotham's *The Nazi Connection*. A memoir rather than a monograph, the book does not, strictly speaking, deal with World War II at all but with the author's activities as a gatherer of intelligence on growing German air power for the British Secret Service during the period 1934-39. Its value for serious students of Nazi Germany, World War II, or military intelligence is slight. Its entertainment value, some of which is, unfortunately, unintentional, is modest. It is amusing to read a description of the portly Goering stuck and cursing in the narrow doorway of a Nuremberg tavern. It is also amusing to read of the disastrous inflation that Germany supposedly experienced in 1930 and of the one-hundred-seventy-two seats in the Reichstag that the Nazis purportedly won in the same year—amusing but also unsettling. Errors such as these abound and make evident the fact that the book was not exposed to historically informed editing.

Aided by his "Aryan" good looks, Winterbotham succeeded in establishing a friendly relationship with Alfred Rosenberg and subsequently used Rosenberg to secure access to other Nazi potentates. He concluded that Germany had aggressive ambitions and that air power would be important in the realization of those ambitions. Of greater interest than these observations, which seem so unremarkable today, are the author's own attitudes toward the Nazi system as he witnessed it in the 1930s. Winterbotham, an Oxonian and member of "polite" English society, was not unsympathetic toward much of what he saw and ultimately regretted that Britain and France had not adopted a policy of heavily armed neutrality while permitting Germany to attack the Soviet Union.

Read Kahn for voluminous detail on German military intelligence and Winterbotham for Winterbotham.

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ROBERT EDWIN HERZSTEIN. *The War that Hitler Won: The Most Infamous Propaganda Campaign in History*. New York: G. P. Putnam. 1978. Pp. 491. \$15.00.

Joseph Paul Goebbels, Hitler's impresario, continues to fascinate historians almost as much as the Führer himself. As chief propagandist and keeper

of the Hitler image, Goebbels was probably the most visible of the major Nazi leaders during the war years. As such, he provides the logical focus for Robert Edwin Herzstein's study of wartime propaganda. Herzstein argues that Goebbels's nihilistic version of National Socialism, including an obsession with heroic death, dominated the media in the Third Reich. "Goebbels gave Hitler his greatest victory of the war, the final conquest of the German people" (p. 110); Goebbels successfully convinced the German people to fight to the end in a "victory or death" *Götterdämmerung* rather than repeat the "shame" of 1918.

Herzstein presents a comparative analysis of the various German media during World War II. His topically arranged narrative discusses in turn Goebbels's ideas, propaganda techniques, wartime cinema, anti-English and anti-Russian themes, and popular reactions. This approach enables the author to concentrate on specific themes and media; it also contributes to a degree of repetition and forced organization of material. Drawing on a wide and impressive variety of published and microfilmed primary sources, as well as many films, Herzstein illustrates in considerable detail how the Nazis attempted to use every available medium for propaganda purposes.

Among the more original sections are those on visual propaganda—graphics, newsreels, and feature films—which are nicely complemented by appropriate illustrations. Because cinema and radio attracted Goebbels's special attention, Herzstein tends to favor these media in his discussion. For example, he summarizes about twenty of the more than thirteen hundred feature films produced in the Third Reich. The weekly newsreels, the *Deutsche Wochenschauen*, probably proved more effective as official propaganda because they included at least some news about the war. But even their potential impact must have decreased when they ignored the epic struggle at Stalingrad in favor of travelogues and shots of happy ice skaters in the winter of 1942-43. By 1944 Germans were referring to such newsreels as fairy tales and even walked out of movie houses before they were shown. Incidentally, much additional evidence marshaled by Herzstein tends to weaken his overall thesis: police reports of increased listening to foreign broadcasts, declining attendance at party functions, and hate letters to Nazi leaders, including a marvelously appropriate note encouraging Hans Fritzsche, the regime's leading radio commentator, to "keep your mouth shut and go to the front, and become a hero!" (p. 409). Popular disenchantment spread as the war dragged on, a fact that the secret police documented in their "reports from the nation," perhaps the most accurate source of public opinion in the Third Reich.

By eschewing analysis in favor of detail, Herz-

stein covers a lot of ground unevenly. Certain areas seem unduly slighted: the press, where Goebbels had to deal with the competition of Otto Dietrich and Max Amann; the significance of such feudalistic rivalries for the success of propaganda activities; and the role of local propagandists. Still, in the discussions of cinematic propaganda, this study complements, but does not supersede, such older works as Zeman and Bramsted, as well as more recent ones by Baird and Steinert. In the final analysis, one wonders how successful Goebbels's wartime propaganda really was. Military victories made good propaganda and built public support for the regime. But, as one local party official noted in late March 1945, "the military situation is . . . not one which will contribute to raising the generally depressed morale of the population" (p. 401). Even earlier, Herzstein admits that "by 1944 Goebbels was making propaganda as much for himself and the leadership as for the masses" (p. 107). A heroic "victory or death" may have attracted Nazi zealots, for rather obvious reasons, but how attractive was such a choice to the rest of the German population? Their alternatives were painfully limited, but survival of the Nazi disaster must have been uppermost in the minds of many Germans. Skillful impresario that he was, Goebbels did not convince all of the German people to follow his own and the Führer's example.

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ALF MINTZEL. *Geschichte der CSU: Ein Überblick*. Wiesbaden: Westdeutscher Verlag. 1977. Pp. 498. DM 29.

If only because of its length and detail Alf Mintzel's new history of the Christian Social Union (CSU) is unlikely to reach the "broad politically interested readership" for whom it is written. For the scholarly audience, on the other hand, its extent and exhaustiveness are assets. Mintzel is an expert and impartial observer of the Bavarian party; few other works on the CSU, apart from Mintzel's earlier organizational analysis, *Die CSU: Anatomie einer konservativen Partei, 1945-1972* (1975), tell as much about its development as this volume does.

Much of the writing on the CSU, academic as well as journalistic, has coupled the party with the bullish personality of its longtime leader, Franz Josef Strauss, or with the unique and esoteric qualities of the Bavarian "character." It is Mintzel's greatest virtue in this study to focus on the CSU itself. Shifting the emphasis away from Strauss, he argues that on the whole individuals have come to count less for the party's direction and future since its early years; from an old-style party of notables (*Honoratiorenpartei*) the CSU has evolved into a

"*Massen- und Apparat-Partei modernen Typs*." In the process it has also lost much of its *bajuwarischen* individuality and developed a conservative-technocratic politics of adaptation suited to Bavaria's postwar industrialization. In these transformations reside the basic causes of the notable electoral success the CSU has enjoyed since the early 1950s. Traditional territorial and even confessional divisions have become increasingly irrelevant to Bavarian voters when measured against the party's accomplishments as "*Staats- und Ordnungs-partei*." In Mintzel's view the CSU's history cannot be separated from its Bavarian context—the real context of a modernized European region, not the picturesque fancy of times past.

Along with the strengths it brings to his analysis, Mintzel's institutional approach also has its blind side; it leaves out the flesh and blood of change. Mintzel tends to portray individuals along one dimension and never assesses conclusively their contributions to the CSU's success. This is especially true of his treatment of Strauss, who, after all, has been a party leader longer than any other West German politician. Mintzel also needs to say more about the ways in which the CSU dealt with the major issues of its three-and-one-half decades in order to explain better how the party broadened its popular base while becoming more bureaucratized and less participatory. The references to the process of becoming a *Massen- und Apparat-Partei*, drawn from Sigmund Neumann's terminology, ultimately take on the sense of formulas of inevitability, not true historical explanations. In the end Mintzel's treatment of the CSU seems, like his earlier study, more *Anatomie* than history. In its topical organization the book is also not historical enough; lacking a basic narrative as prologue it becomes somewhat choked with repetition and cross-references. Nevertheless, on balance, the virtues of this study outweigh its shortcomings. Further writings on the CSU should build upon Mintzel's foundation.

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ANNE JACOBSON SCHUTTE. *Pier Paolo Vergerio: The Making of an Italian Reformer*. (Travaux d'Humanisme et Renaissance, number 160.) Geneva: Librairie Droz. 1977. Pp. 300.

The flight of Pier Paolo Vergerio to Switzerland in 1549 and his abandonment of Roman Catholicism were shocking events to his Italian contemporaries. Bishop of Capodistria since 1536, Vergerio had been a lawyer, then twice papal nuncio under Popes Clement VII and Paul III, ardent proponent of a council, and friend of many well-known and highly placed Italians. His apostasy resulted in

inquisitorial proceedings against his suspected adherents and the predictable blackening of his reputation by enemies like Girolamo Muzio. Although negative judgments of him by later Catholic historians are understandable, he has not found strong defenders among others since he neither joined an established Protestant church nor progressed in the direction of religious radicalism, rationalism, or antitrinitarianism. His literary activity until his death in 1565 was mainly aimed at Italians whom he hoped to convert to purified, reformed Christianity.

Anne Jacobson Schutte's book, the 1977 Brewer Prize Essay of the American Society of Church History, examines the first fifty-one years of Vergerio's life to see him in his Italian context and to gain deeper knowledge of Italian culture during the Reformation period. The author succeeds splendidly in both aims. Going far beyond any of her predecessors, she draws on vast amounts of archival material, printed sources, and older and recent scholarship in this complex work. She first discusses Vergerio's secular and ecclesiastical career; she then reconstructs his religious and intellectual development and tries to depict the world in which he moved, from the courts of popes and kings down to the clans of Capodistria and their unsavory feuds.

Central to this rich book is its convincing argument that Vergerio was not a crypto-Protestant before leaving Italy but a man won over to the ideals of Italian Evangelism, which he tried to put into effect in his diocese. His attempts at reform, which are reconstructed here most imaginatively from scattered evidence, gave his enemies cause to accuse him of heretical leanings. Schutte shows how Vergerio gradually had to confront an environment hostile to what he considered sound Catholic teaching and maintains that he had his *Turnerlebnis* separating him from the Catholic Church in 1548 at the deathbed of Francesco Spiera, who recanted after being accused of heresy only to despair because he thought he had betrayed God.

The author's style is exemplary. Besides being beautifully written, her book is a masterpiece of careful and subtle argumentation, frequently presenting possible alternative explanations and evaluating their merit. Free of any misplaced sympathy, she examines the development of Vergerio from a Catholic reformer to "a uniquely Italian reformer." In the process she touches many themes bearing upon the effect of the Reformation on Italy and always discusses them with great sensitivity and insight. This book is a major contribution to our knowledge of sixteenth-century Italian religious history.

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ALEXANDER J. DE GRAND. *The Italian Nationalist Association and the Rise of Fascism in Italy*. Lincoln: University of Nebraska Press. 1978. Pp. x, 238. \$12.50.

Alexander J. De Grand argues that "Italian nationalism, as elaborated between 1903 and 1923, was one of the crucial ingredients in the mix which eventually became fascism and that its influence steered fascism in the direction of traditional conservative authoritarianism" (p. ix). De Grand's compact study effectively traces the evolution of this "new" nationalism from its modest beginnings in 1903, with the founding of Enrico Corradini's periodical *Il Regno*, to the fusion of the Nationalist and Fascist Parties in 1923.

The Nationalist Association's aggressive posture during the Libyan enterprise and the First World War and its role in facilitating Mussolini's advent to power by acting as a bridge between a conservative bourgeoisie disenchanted with parliamentary government and a fascism as yet chaotic in ideas, are neatly related to the ideology and initiative provided by Corradini, Luigi Federzoni, and Alfredo Rocco. Perhaps the most perceptive, if all too brief, part of De Grand's work is his account of how, having helped salvage Mussolini in his moment of peril after Matteotti's murder, Federzoni and Rocco impressed on the new regime their view that the state, not the Fascist Party, should be "the primary engine of political and social action" (p. 173).

But the state as understood by Federzoni and Rocco was not the reforming liberal parliamentary state of the Giolittian era. It was precisely against the Giolittian state that Corradini preached his doctrines of a "proletarian" Italy, whose more perfect unity and greatness were to be achieved, not by making concessions to "antinational" socialism, but only through imperialist ventures. These ventures, subjecting individual interests to "national egoism," would put an end to domestic class struggle: war abroad to end the war within. De Grand delineates clearly how Corradini's ideas, amplified and refined by Federzoni and especially Rocco, eventually became part of the doctrinal baggage of fascism. He is less successful in his attempt to assign definitional terms to Nationalist thought and attitudes.

On a single page (p. 23), Federzoni is characterized as being "from the outset a conservative traditionalist," a "traditional monarchist," orthodox in politics and religion, and "closer to the classical conservative position than the other leaders of the Nationalist movement." It may be splitting hairs to contend that these characterizations are not synonymous; but when Federzoni is classed among the "new conservatives" (p. 27) and depicted as the real leader of a movement that by 1912 had "made a beginning in the elaboration of

the new authoritarianism" (p. 41), one may wonder what De Grand means by his assertion that the Nationalists steered fascism in the direction of traditional conservative authoritarianism. To make matters more confusing, we are told that in 1914 the Nationalists "realized that modern society was too complex for traditional authoritarianism" (p. 49), the same authoritarianism into which fascism was purportedly steered by the Nationalists. Furthermore, De Grand states that as late as 1920 the Nationalists were prepared to support Giolitti because of their "confidence that Giolitti could restore the solidity of the constitutional bloc" (p. 117). How can one square authoritarianism, old or new, with constitutionalism?

Despite these criticisms of the author's descriptive terms, as a whole his study is worthy of attention, not least for its massive documentation (47 pages of notes supporting 179 of text) including much material hitherto unexplored.

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PIER GIORGIO ZUNINO. *La questione cattolica nella sinistra italiana (1940-1945)*. (Saggi, number 172.) Bologna: Il Mulino. 1977. Pp. 226. L. 6,000.

This volume presents a concise and illuminating picture of the historical antecedents of the much discussed *compromesso storico*, which has recently become a central subject of negotiation between Italian Catholics and Communists. The period Pier Giorgio Zunino considers is the Second World War; his findings, however, have implications that go far beyond that period and illuminate the events of both the postwar era and the present day.

Using a wide range of sources, including unpublished archival material, published documents, memoirs, letters, and personal communications, the author constructs a picture of the response of the Italian Left to the vexing wartime "Catholic question": what to do with the Church in a post-fascist Italy; how to treat with the collaborationist Vatican hierarchy as well as with potential supporters among the Catholic masses. The first several chapters present a roughly chronological picture of developments between September 1939 and July 1943, not only in Italy but among exiled intellectuals and party functionaries in America, Western Europe, and the Soviet Union. Zunino does much to delineate the discussions that such exiled luminaries as Gaetano Salvemini and Count Carlo Sforza carried on among themselves and with the U.S. State Department; he also considers the rather idiosyncratic position of PCI members who found refuge in New York and published journals there. In addition, the author sheds much

light on the emerging position of Communist Party head Palmiro Togliatti, who in a series of radio broadcasts from his Moscow exile began to elaborate his stance toward the Catholic Church along lines laid down in the 1920s and during the Popular Front era of the mid-1930s.

By the time Togliatti returned to Italy early in 1944, his position was well defined: owing to the risk of civil war and the dominance of the Western Allies, a revolutionary policy was not to be followed; rather, the PCI would seek to build an antifascist and democratic bloc through alliances with other mass parties. The Catholic Church and the nascent Christian Democratic movement became especially important in this blueprint, and the PCI leadership sought to build bridges both to the left wing of the DC and to the main party leaders while attempting to isolate both the Catholic conservatives who formed part of the movement and the Vatican hierarchy who attempted to guide it. This program, embodied in the *Svolta di Salerno* is analyzed in depth, and Zunino argues that in contrast to the idealism and anti-Vatican intransigence exhibited by Italian Socialists and the *Partito d'Azione*, Togliatti's plan represented a realistic and rather shrewd attempt to deal with the political actualities of wartime and postwar Italy.

Succeeding chapters concern the fate of this unity program in national politics as well as in trade union activity, youth federations, women's organizations, and the armed resistance bands operating in the German-occupied Italian North. Some attention is paid to wartime developments in local zones and areas, which, as the author notes, have been the subject of much historiographical study and memorializing. It would have been useful, however, to devote more attention to these questions and to the contrasts between the national leadership and the local rank and file. One never learns the specific attitudes of workers in northern Italian factories toward the *Svolta*, although the author alludes in a rather general way to splits between "sectarians" and "opportunists." Nor do we learn enough about the specific programs and responses of such leftist forces as the Socialist Party (PSI) and the *Partito d'Azione*. Owing perhaps to the availability of documentation and to partisan political choice, the author deals more thoroughly with the Communist response to the Catholic question than that of the Left as a whole.

In a similar manner, we are not given an adequate treatment of Catholic reaction to the programs and initiatives of the Left and of the PCI. Such analysis would assist the reader in assessing the specific ways in which Togliatti's unity program failed. It seems clear that the main sources of its defeat lay both in Catholic resistance to Togliatti's initiatives as well as in splits that the unity

program produced within the Communist movement and the rest of the Left. The author outlines the general manner in which the Church hierarchy resisted any fundamental union with the Left while making concessions in several limited areas, such as that of trade union activity. He also briefly discusses the ways in which the unity program undermined the solidarity of the Left itself, driving a wedge between those who resisted the directives of the party leadership on ideological grounds and those who supported a more active policy of alliance with the Catholics. Both sets of issues are still present in debate about the *compromesso storico*—the contemporary version of Togliatti's unity program. Proponents of the *compromesso* will need a more thorough understanding of the failures of the wartime program than is presented here if they intend their current efforts to succeed.

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MASSIMO FIRPO. *Antitrinitari nell'Europa orientale del '500: Nuovi testi di Szymon Budny, Niccolò Paruta e Iacopo Paleologo*. (Pubblicazioni del "Centro di Studi del Pensiero Filosofico del Cinquecento e del Seicento in Relazione ai Problemi della Scienza," Serie I, Studi, number 8.) Florence: La Nuova Italia Editrice. 1977. Pp. xiv, 414. L. 10,000.

This book offers four new antitrinitarian texts with an intriguing history. In 1578 the radical theologian Szymon Budny published them in Lithuania, but no printed copies survived. In 1730 the *Bibliotheca Uffenbachiana* recorded the existence of a hand-copied version, which eventually passed to a Hamburg library. During World War II the manuscript disappeared, only recently surfacing in the USSR, and it has now been deposited in East Berlin's Deutsche Staatsbibliothek. Massimo Firpo has produced a superb edition, meticulously annotated and with a 270-page introduction, placing the new material in the context of what is known about antitrinitarianism. His scholarship, which sets a high standard for future work, is an important addition to recent publications by Italian and East European scholars.

The texts come from the years between Servetus's execution and Fausto Sozzini's formulation of a unitarian orthodoxy, during which Western antitrinitarians could find a precarious security only in Poland, Moravia, and Transylvania. This was an era of tragic conflicts within the radical movement over baptism, the legitimacy of secular authority, the unity of the Godhead, the adoration of Christ, and the search for a common ground with Judaism and Islam—questions that transformed even some of the radicals into persecutors. Sources for the

period are often fragmentary and difficult to locate, and there are numerous problems of dating, authorship, and influence. A thorough grasp of patristics and a knowledge of East European languages are essential for students of the subject. But the boldness of the radicals' theology, their preference for ethics and free will over predestination, and their debates over toleration, pacifism, social justice, and union with non-Christian religions make this an inviting field for research.

The present texts address themselves exclusively to the christological debate. The first, Budny's *Ad argumenta Stanislai Grochovii, quibus duas in uno eodemque Christo nititur ostendere naturas, brevis e sacris Lileris responsio* (1575), is part of a protracted polemic with Josias Simler and Johannes Wigand, and Firpo's analysis of the debate is a valuable contribution. The work belongs to the period in which Budny was developing a unitarian, non-adorantist christology. The surviving fragment of a second Budny text, *Brevis demonstratio quod Christus non sit ipse Deus qui est pater nec eidem aequalis* (1575), is also published here, confirming Stanisław Kot's earlier reconstruction of the work and illuminating Budny's efforts to win over John Foxe. Little of Budny's prolific output has survived, but the documents published here and his other extant works show him to have been a formidable biblical exegesis with whom specialists should become better acquainted.

Still more exciting are the collection's two other new texts, the anonymous *Assertionum Iosiae Simleri de duabus in Christo naturis confutationis* (1576) and Niccolò Paruta's *De uno vero Deo Iehova fragmenta quaedam disputationem* (date uncertain). Firpo earlier ascribed the *Assertionum* to Gianpaolo Alciati; he now believes the author to have been Jacopo Paleologus, the Greco-Italian theologian whom Kot called "the true originator of the [unitarian] movement, . . . one of the boldest thinkers of his age." If Firpo's new attribution is sustained, our knowledge of the early evolution of Unitarianism has been much enlarged and a stronger link established between Paleologus and Budny, in whose defense the *Assertionum* was written. The Paruta text represents the only known work of this shadowy Venetian exile who lived in the Hutterites' Austerlitz colony in the 1560s. The erudite, tightly reasoned eleven propositions of *De uno vero Deo Iehova* correspond precisely to Paruta's antitrinitarian theses as recently reconstructed by Valerio Marchetti, and Firpo cautiously argues that the work was written before 1568, the year that Blandrata and Ferenc Dávid published in Transylvania *De falsa et vera unius Dei patris, filii et spiritus sancti cognitione*, hitherto accepted as the seminal statement of Polish-Transylvanian antitrinitarianism. Firpo's dating would tend to make Paruta, not

Blandrata and Dávid, the first systematic formulator of East European radical theology. An important unanswered question is the connection between *De uno vero Deo Iehova* and the twenty theses of Paruta's associate, Dario Sozzini, which were disseminated in Poland in 1562, when the split between Calvinists and antitrinitarians was rapidly widening. Paleologus's writings reveal that Paruta was not executed at Venice in 1567, as reported by some modern authorities, but lived in Moravia, Poland, and Transylvania until about 1581. Publication of *De uno vero deo Iehova* is a major event in the study of the radicals, and Paruta himself has emerged as a genuine leader of the movement and a more original thinker than hitherto suspected.

We are still far from knowing the full story of the East European radical Reformation. In the area of pure theology, to which Firpo has confined himself, we need to explore more thoroughly the possibly far-reaching influence of Piotr of Goniądz and Andreas Dudith. And there is a large body of works by Paleologus known only by title (Firpo lists them on page 183) that should be tracked down and published—above all the manuscripts confiscated when Paleologus was arrested in 1581, which are still in the archives of the Holy Office.

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IVÁN T. BEREND and GYÖRGY RÁNKI. *East Central Europe in the 19th and 20th Centuries*. Budapest: Akadémiai Kiadó. 1977. Pp. 163. \$12.00.

This book is designed to be a popular comparative history of East Central Europe in the nineteenth and twentieth centuries. Its authors, Iván T. Berend and György Ránki, are well-known Hungarian economic historians who have written on a wide range of topics in Hungarian and East European history. Two other works by the same authors have appeared in English, *Hungary, a Century of Economic Development* (1974), and *Economic Development in East Central Europe in the 19th and 20th Centuries* (1974).

This work is popular in that the authors assume the reader has very little background. They begin with basic facts—what East Central Europe is and who lives there—and then go on to brief, very condensed, descriptive chapters that stress pattern over detail. There are almost no footnotes; and, although the authors use many statistics, they rarely cite their sources. The bibliography is slight, more a list of recommended readings on the topic in major European languages than an indication of the sources used. Many important works are omitted. The lack of an adequate schol-

arly apparatus, however, does not mean that this work is shallow. In fact, the very opposite is true. Such a book could only be written after many years of research, teaching, and writing about East Central Europe. Portions of the book are, in fact, simply condensations of parts of the authors' earlier works: the section on the Second World War, for example, is an abbreviated version of chapter thirteen of *Economic Development in East Central Europe*.

The authors' approach is comparative. Instead of discussing each country separately, they describe the development of the area as a whole and of its subregions, giving information on individual countries when relevant, seeking patterns rather than chronology. A general reader may need to have a standard text in hand in order to follow the political history. Most studies of East Central Europe center on one country, nationality, empire, or region. The variety of languages and historical traditions, the diversity in economic and political development discourage most scholars from attempting large-scale comparative studies, although almost everyone working on East Central Europe is by necessity a comparativist. Most contemporary comparative works are so in scope rather than approach, collections of articles by specialists with an introduction and conclusion drawing some comparisons. Although not everyone may agree with all of Berend and Ránki's generalizations, they do make order out of a welter of details and preconceptions, giving the general reader patterns to remember and the specialist statements against which he can test his own observations and conclusions. Unfortunately, the authors do not always define terms clearly. This is, for example, supposed to be a study of the modernization of East Central Europe, yet nowhere do they define modernization. The authors are also somewhat vague in their use of the terms nation, nationality, people, and minority.

Berend and Ránki are most familiar with Hungarian history and frequently use Hungarian examples to illustrate points. Hungary therefore receives the most detailed treatment. Poland and Rumania less, and the Balkan countries the least. There is in fact an anti-Balkan bias built into the book, which lays unnecessary stress on the "backwardness" of the Balkan area. Political history of East Central Europe is condensed to a mere sketch, and diplomatic history rarely mentioned. The authors concentrate on economic and social history, tracing the origins and evolution of the working and middle classes, capital formation, industrial growth, demography, the economic roots of anti-Semitism, the "peasant question," and the impact of modernization.

The book is divided chronologically and topi-

cally. Part one deals with the years between 1815 and 1914, part two with 1914 to 1945. Each has separate chapters on political, economic, and social development. Part one also includes a chapter on cultural history. The translation is generally smooth, although some phrases are awkward, such as "East Central Europe is notoriously a multinational region" (p. 15). There are also some serious typographical errors, for example, Vumicis instead of Kumičić (p. 72). There is a small section of photographs. Intended as an introductory study, this book should be of interest to both the general reader and the specialist.

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LJUBOMIR DURKOVIĆ-JAKŠIĆ. *Ż dziejów stosunków jugosłowiańsko-polskich, 1772-1840* [From the History of Yugoslav-Polish Relations, 1772-1840]. (Matica Srpska-Ossolineum, Studia i rozprawy z historii stosunków jugosłowiańsko-polskich.) Translated from Serbo-Croatian by HALINA KALITA. Wrocław: Ossolineum. 1977. Pp. 280. 80 Zł.

This work is the Polish translation of the author's *Jugoslovensko-polska saradnja, 1772-1840*, originally published in 1971. With the exception of an introduction that briefly surveys the course of Polish-Yugoslav relations as well as Ljubomir Durković-Jakšić's long-term interest in this field, no editorial changes have been made in this edition. The book has five major chapters, organized more or less chronologically: (1) the period of the Polish Partitions, 1772-95; (2) the projected activities of the Polish legions in the Yugoslav lands, 1797-1809; (3) the Karageorge uprising, 1804-13; (4) the Serbian renaissance, 1815-29; and (5) the November Insurrection and the great emigration, 1830-40. The author further breaks down the chapters into sixty-one smaller sections, each meant to cover a specific theme, ranging in length from three lines to nineteen pages.

In its present form the book represents the results of many years of seeking information regarding its broad topic. For this the author must be commended. Essentially, however, the work is a compendium of all the materials that Durković-Jakšić could discover dealing even peripherally with his subject. Both he and the editor admit that little of significance transpired in Polish-Yugoslav relations prior to ca. 1840, which marks the beginnings of Prince Adam Czartoryski's involvement in this area. Why, then, was this book written? It does not serve as an introduction to Czartoryski's activities, nor does the author make any attempt to evaluate his material to establish or to support a thesis. The result is a very uneven work that often

treats frivolous subjects, juxtaposing these to important topics. For example, at one point we learn that in 1837 the English vice-consul's wife enjoyed dancing the mazurka, much to the astonishment of Prince Miloš Obrenović (p. 209). A few pages later we have a treatment of the relations between Petar II Petrović Njegoš of Montenegro with the Poles (pp. 211-17), followed by information concerning a journey by the Croatian nationalist, Ljudevit Gaj, during which he passed through Poland (pp. 220-27). In several chapters Durković-Jakšić deals with the journalistic coverage given to the Poles in the Yugoslav press and vice versa. Again, however, no effort is made to evaluate material. What emerges is simply a summary of articles in which information concerning the November Insurrection may be commingled with a story listing the numbers of books in various Polish libraries.

This book is basically a collection of materials, arranged chronologically. As an encyclopedic reference, Durković-Jakšić's work does serve the historian or researcher well. The extensive index allows the user to tell easily whether a given figure was even peripherally involved in Polish-Yugoslav relations and in what context. This is not, however, a book to read for an analysis of this subject. Writing originally for a Yugoslav audience, the author makes tacit assumptions of historical knowledge and proceeds to bombard the reader with an uncritical, unsifted recitation of factual material. Sadly, although noting these problems in his introduction, the editor, Ludwik Bazyłow, did nothing either to enliven the book or to render it more useful for the audience for which the translation was intended.

ROBERT A. BERRY
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GEORGE B. LEON. *The Greek Socialist Movement and the First World War: The Road to Unity*. (East European Monographs, number 18.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1976. Pp. vii, 204. \$12.00.

The crucial impact of foreign developments on the course of modern Greek history has been widely noted. To historians of modern Greece, the examination of the so-called foreign factor is considered practically indispensable for understanding and explaining nearly all of the important changes that have occurred in that country since the European powers first intervened to secure Greek independence from Ottoman rule over a century and a half ago. Although certain elements of exaggeration do appear in this type of analysis, notably the tendency to saddle outsiders with responsibility for

the frequent upheavals and crises in Greek history, there can be little doubt that this interplay of national and international factors is a phenomenon to be seriously reckoned with.

With regard to the Greek socialist movement, for example, George B. Leon's brief but perceptive monograph demonstrates quite convincingly that it was the turmoil in Greece precipitated by the First World War that served to radicalize and unify its hopelessly fragmented and feuding factions. This conclusion, reached after intensive research, should be of considerable interest for historians of European socialism; it contrasts sharply with the experience of the European socialist parties, which were dealt a shattering blow as a result of the outbreak of the Great War.

In 1914 the Greek socialists were a weak, divided group of intellectuals and workers whose political influence was altogether negligible. By no stretch of the imagination could they be called a party. And when war erupted in Europe the divisions between the socialists deepened, for they had to choose sides on the question of intervention versus neutrality—the issue that developed into the “national schism,” as it is called by Greek historians who have chronicled its catastrophic consequences.

Venizelos, the nationalist leader of the governing Liberal Party, favored Greece's entry into the war on the Entente's side. His goal was to achieve the *Megale Idea*—the realization of Greece's age-old dreams to acquire southern Albania, the Aegean Islands, Thrace, western Anatolia, and possibly even Constantinople. Confronting Venizelos stood King Constantine, who believed the Germans would prevail and therefore wished to keep Greece neutral. “Patriotic socialists” such as Drakoulas and Yiannios supported Venizelos's irredentist policies. Others, led by Benaroya of the Socialist Labor Federation of Salonika, adopted a properly socialist, neutralist, antiwar position, but this, ironically, amounted to an endorsement of the king and royalist forces whose policies they opposed as being reactionary on all domestic issues.

Greek socialist delegates at the Inter-Allied Labor and Socialist Conferences held in London during the war impressed no one but themselves with their ceaseless infighting and bickering. Consequently, Venizelos decided it was high time to bring about the unification of the Greek labor and socialist movements so they could more effectively champion his war aims in European socialist circles, which he believed were destined to play an important role in any future peace conference. The Greek socialists—increasingly radicalized after the Russian Revolution and galvanized by the possibilities inherent in the country's internal crisis arising out of the strains of war—agreed to go

along, hoping to turn Venizelos's foreign policy needs to their advantage. Finally, in November 1918, the Greek Socialist Party was born under circumstances so bizarre and suspect that, as Leon puts it in his last sentence, “the rapid bolshevization of Greek socialism should occasion no surprise” to anyone.

This monograph is a significant contribution to the social history of Greece—a field in which very little scholarly work has been done. It supersedes all previous accounts of the origins and formation of the Greek Socialist Party.

DENNIS N. SKIOTIS
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EVANGELOS AVEROFF-TOSSIZZA. *By Fire and Axe: The Communist Party and the Civil War in Greece, 1944–1949*. Translated by SARAH ARNOLD RIGOS. New Rochelle, N.Y.: Caratzas Brothers. 1978. Pp. xi, 438. \$15.00.

Political strife is no stranger to twentieth-century Greece, but it reached its greatest intensity in the 1940s. A complex series of struggles that had outside powers directly and indirectly involved, this period remains a subject of political and historical controversy. It began with the nation heroically throwing back Italian invaders in late 1940. The glory of victory proved short lived as Greece in the spring of 1941 succumbed quickly to superior German armies. The Axis occupation, one of the harshest in Europe, stimulated the formation of resistance organizations, the strongest of which was the National Liberation Front, or EAM. With the Greek Communist Party (KKE) dominating its leadership, EAM fought against both foreign occupiers and rival resistance groups as it sought to play a primary role in the development of liberated Greece's political and social institutions. The unsuccessful campaign of the KKE to emerge dominant is customarily divided into three phases. The “first round” occurred in anticipation of liberation, when EAM battled other resistance organizations from October 1943 to February 1944. After liberation the “second round” erupted in early December 1944 and lasted ten weeks, as leftist forces challenged British and Greek government units. By 1946 the popular front format of the Left had disintegrated and hard core elements of the KKE launched the “third round,” which lasted until October 1949 and the guerrillas' proclamation of a “temporary cessation” of hostilities. By then the Greek Civil War had become an integral part of the Cold War.

Evangelos Averoff-Tossizza tackles the main issues of the second and third rounds in this study, which has appeared earlier in Greek and French

editions. A veteran conservative politician, Averoff-Tossizza has served in the several governments of Constantine Karamanlis, as foreign minister from 1956 to 1963 and as minister of national defense since the fall of military dictatorship in 1974.

Averoff-Tossizza culled most of his information from standard published sources to produce this book. One wishes that, since the author participated politically during the period under review, he might have devoted more space to his own experiences, thereby offering a political memoir. Instead, the volume becomes a journalistic synthesis, reflective of the political Right's attitudes toward these critical years. There is no index; the bibliography is limited; footnotes are scant and barely informative; and the English translation could have been improved stylistically. On the other hand, the thirty-five pages of photographs, some of them rare, are interesting. Thus, the end result does not approach the broad claim of the publisher in the preface, that the book "is valuable both as a historical work and as a primary source. . . ."

It should be noted, too, that Averoff-Tossizza hardly refers to the one issue that galvanized many of the disparate elements of the Greek population: opposition to the restoration of the monarchy. This crucial problem plagued Greek leaders as well as British policy makers, whose influential role in the Greek resistance is understated. Despite some insightful and objective statements on KKE policies and leadership during the second and third rounds, Averoff-Tossizza, because of the limitations of his sources and his political position, cannot possibly provide an authoritative account of "the Communist Party and the Civil War in Greece, 1944-1949," the alluring subtitle of his book.

The passage of time, the restoration of civilian rule in 1974, the legalization of the KKE, and the accessibility now of relevant archives in Great Britain, the United States, and Germany are all contributing to renewed interest in and greater understanding of Greece during the 1940s. The positive impact of recent scholarship is already apparent and new studies by Greek and non-Greek scholars are eagerly awaited. These publications more than likely will shove earlier books, such as Averoff-Tossizza's contribution, into the dustier corners of bookshelves.

S. VICTOR PAPACOSMA
Kent State University

NADA KLAJČ. *Društvena previranja i bune u Hrvatskoj u XVI i XVII stoljeću* [Social Ferment and Uprisings in Croatia in the Sixteenth and Seventeenth Centuries]. Belgrade: Nolit. [1976]. Pp. 308.

Since World War II a great deal has been written on social unrest and peasant uprisings in Croatia. Among the published materials there are a few valuable documentary and monographic works. One of those who has done some of the best work in social history is Nada Klaić, a prominent expert on Croatian medieval and early modern history. Her works have elicited lively polemics among Yugoslav and Soviet historians concerning her interpretations of Croatian feudalism and the peasant uprising of Matija Gubec in 1573.

Klaić disagrees with those who think that the causes of peasant uprisings should be sought exclusively in the economic sphere. She admits that, until several years ago, she herself was guilty of this narrowness of focus and attributes it to the influence of Rudolf Bičanić, the well-known Croatian economic historian, who was the first to discern "capitalistic" elements in the 1573 peasant uprising. This economic approach to the investigation of the peasant uprisings, she writes, appeared attractive and was adopted by many Yugoslav historians. According to Klaić, however, economic reductionism is undialectical and does not conform to the Marxist interpretation of the historical process. In her opinion the history of the uprisings must be investigated in its totality.

In the introductory chapter to the book under review, she explains the differences in the historical development of the three Croatian provinces, Dalmatia, Croatia, and Slavonia. The main body of the book is divided into two parts—sixteenth century and seventeenth century—and each part is subdivided by provinces. In the first part of the book Klaić discusses the basic causes of social unrest in medieval Dalmatia, the Hvar uprising of Matej Ivanić, the characteristics of Croatian and Slavonian society, economic conditions that contributed to the development of social movements, and the "crusading war" against the Gregorić and Gubec uprising in 1573. Interestingly enough, Klaić minimizes the importance of the Ivanić uprising. She says that Ivanić did not merit the honor given him by the Partisans during the National Liberation War because he was hardly more than a Venetian condottiere who received a decoration for his services from the Venetian government.

The most important social movement in the Croatian lands in this period was the peasant uprising in 1573. Klaić seeks to answer two questions concerning the uprising: why was it a failure, and how much did it cost Slavonian society? The first question, she says, is not difficult to answer and even contemporaries recognized the problem: the insurgent infantry was no match for the cavalry of the adversary. Consequently, the uprising was doomed to failure. The cost of the uprising, she argues, is difficult to ascertain because of the un-

reliability of the available evidence. The references to many thousands of dead on the battlefield in Zagorje are undoubtedly exaggerated. The nobility for its own reasons found it expedient to exaggerate or distort the facts. Even official reports on the number of insurgents and the extent of destruction are often inaccurate. Klaić thinks that the number of dead probably did not exceed one thousand.

In the second part of the book Klaić discusses such questions as the struggle of the Croatian nobility against the Habsburgs (the Nikola Zrinski-Franjo Krsto Frankapan conspiracy of 1671), disturbances among the inhabitants of Krajina (including the Vlach rebellions), unrest among the city dwellers of the royal towns, and the struggle of the feudal towns for their "rights." Croatian historians have extensively investigated the Zrinski-Frankapan conspiracy, yet they continue to disagree on some of its aspects. In examining this question, Klaić concludes that if we look at the conspirators "as children of their time, with all positive and negative characteristics—then we can assess their historical role only in a positive way. But their final imprudent move—that is, the conspiracy and uprising—because of its dire consequences for Croatia, must be assessed negatively" (pp. 138–39).

Klaić's treatment of Croatian social unrest in the sixteenth and the seventeenth centuries is intended for the wider reading public, and yet it is a sophisticated and a scholarly work based on earlier and freshly uncovered primary sources and the latest monographic literature. What is of particular importance is that the book contains the most recent Croatian interpretation of social unrest in Croatia in the sixteenth and the seventeenth centuries. The author's stature and place in Croatian historical scholarship adds to the work's significance.

WAYNE S. VUCINICH
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BOGDAN DENIS DENITCH. *The Legitimation of a Revolution: The Yugoslav Case*. New Haven: Yale University Press. 1976. Pp. xi, 254. \$15.00.

With this volume Bogdan Denis Denitch establishes the legitimacy of the Yugoslav Revolution through a sociological and political delineation of its course since the Partisan struggle of 1941–45. In so doing, he also attempts to respond to the more and more frequently asked question, "After Tito, what will happen to Yugoslavia?" Based on his detailed analysis of the thus far successful legitimation of the Yugoslav Revolution, Denitch's answer is that no immediate or radical shifts will occur after Tito's passing and that continued prag-

matic development along currently indicated lines seems assured. As yet, the author's view is shared by only a minority of those with expertise in contemporary Yugoslav affairs, but more books of this caliber will aid greatly in proving its credibility and expanding its support.

After an opening chapter reviewing some of the pertinent Western and Yugoslav literature on modernization and legitimation in Yugoslavia, the Soviet Union, and Eastern Europe, Denitch examines Yugoslavia's two major problems, multinationalism and underdevelopment, which the Communist revolution had to surmount to succeed. The foundation for the unique solutions offered by the revolutionary elite since 1945 is the Partisan guerrilla war. For example, multi-ethnic cooperation in the Partisan ranks helped in overcoming some of the historic national vendettas of the Balkans and facilitated the institutionalization of multinationalism in postwar Yugoslavia through the constitutions, federal leadership and bureaucracy, party (League of Yugoslav Communists), and military. Supporting his analysis with the results of numerous sociological studies, Denitch concludes that Yugoslavia is emerging as a politically multi-ethnic state with no dominant nationality. But, as he points out, official multinationalism is not the only factor contributing to the legitimation of the revolution. Nor, after more than three decades of tortuous trial and error on the part of the Yugoslavs, can this multinationalism be understood fully in isolation from other aspects of Yugoslav socialism, such as an independent and nonaligned foreign policy, a market economy with indicative planning, and, "socially, a new political culture based on self-management in the form of workers councils, self-managing bodies in institutions and in the communes" (p. 151).

Although Denitch uses a comparative approach and treats Yugoslavia as a case study and not a yardstick, he also raises some important questions about contemporary social science methodology and theories of social change and modernization. There is, for example, a problem with advancing the successful Yugoslav system of self-management as a dynamic alternative model to Soviet or Chinese Communism for other developing countries to adopt or as a basis for comparison with other case studies. Denitch asserts that self-management is not just a form of social organization, but rather that it, fused with other complex factors, is the actual social fabric of Yugoslavia. It is, therefore, possibly unique and nontransferable. Denitch's stated purpose is to provoke more questions than he answers. He clearly and concisely presents a solid analysis of the legitimation of the Yugoslav Revolution that contains serious implications for scholars dealing with contemporary

Yugoslavia, other national Communist states, and the Third World.

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ȘTEFAN HURMUZACHE and VASILE ARIMIA, editors. *Independența României: Documente* [The Independence of Rumania: Documents]. Volume 1, *Documente și presă internă* [Documents and the Domestic Press]; volume 2, part 1, *Correspondență diplomatică străină, 1853-1877 mai* [Foreign Diplomatic Correspondence, 1853-May 1877]; volume 2, part 2, *Correspondență diplomatică străină, 1877 mai-1878 decembrie* [Foreign Diplomatic Correspondence, May 1877-December 1878]; volume 3, *Presă străină* [Foreign Press]. Bucharest: Editura Academiei Republicii Socialiste România. 1977. Pp. lii, 420; lxii, 428; liv, 381; liii, 338. 33 L.; 34 L.; 30 L.; 29 L.

This valuable set of documents illustrates a variety of viewpoints on the 1877-78 war in which Rumania won national independence. Documents in this collection supplement the nine volumes (*Documente privind istoria României: Războiul pentru independență*) also published by the Rumanian academy in the mid-1950s. In contrast to the earlier volumes, which emphasized Rumanian internal affairs during the war, the present set of skillfully edited and hitherto largely unpublished materials focuses on foreign impressions—that is, contemporary writings by non-Rumanian observers as well as by Rumanians outside the country. Archives and libraries in Austria, Hungary, England, Italy, Belgium, and Russia—as well as in France, Germany, Yugoslavia, the United States, Turkey, and Greece—were explored in selecting materials for publication. Additional documents were chosen from the Rumanian state archives (for example, from the *Casa regală* and ministry records), from regional archives in Transylvania and elsewhere, and from newspaper files in state and local libraries. Also, foreign press reports from thirty-seven periodicals were selected for inclusion on the basis of their conformity with “established and unanimously recognized historical truths” (3: xxviii). In all, 871 documents from 1853 to 1879 are in these volumes; 71 percent of the items are dated 1877, 16 percent 1878, and 7 percent 1876. Non-Rumanian documents appear in their original languages, with a Rumanian translation. Each volume includes an introductory narrative of the contents: volume one, Rumanian documents and newspaper accounts, by the late Traian Lungu; volume two, foreign diplomatic correspondence, by Dan Berindei; and volume three, foreign newspaper dispatches, by Mircea Spiridonceanu.

Some documents in these volumes illuminate

Rumania's participation in the war from the standpoint of strategy and tactics as well as both Russian and Rumanian political-military objectives. Rumanian leaders, in particular, are shown in a positive light—effectively marshaling their country's resources to equip and to feed the army, to improve troop morale, and to achieve national goals. Other documents reveal connections between Rumania's war for independence, on the one hand, with the Bulgarian national liberation movement south of the Danube, and, on the other hand, with the Rumanian national movement in the Habsburg Empire. Eyewitness reports, at times truncated, from Rumanian and foreign newspapers help in understanding public opinion at home and abroad concerning Rumanian aims and accomplishments; European newspapers seemingly supported and encouraged Rumanians in their quest for independence. Also, press reports and diplomatic correspondence in these volumes highlight Rumanian efforts to secure recognition of national independence from the great powers of Europe—independence which was won on the field of battle one hundred years ago.

FREDERICK KELLOGG
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BÉLA KÖPECZI, editor. *L'Autobiographie d'un prince rebelle: Confession et Mémoires de François II Rákóczi*. Budapest: Editions Corvina. 1977. Pp. 697.

This edition of the major writings of Ferenc II Rákóczi provides a useful documentary companion to Béla Köpeczi's major work, *La France et la Hongrie au début du XVIII^e siècle* (1971). In this volume Köpeczi has pieced together Rákóczi's *Mémoires* and *Confession* in a way that provides a relatively coherent autobiography of the Transylvanian prince who led the Hungarian “war for independence” against Austria from 1703 to 1712. The *Mémoires*, which cover only the years of the rebellion, make up the central and most substantial part of the book. Composed in French and revised substantially during Rákóczi's later years of exile in Turkey, they were published at The Hague in 1739 along with the *Mémoires* of Nicholas Bethlen. The *Confession* was originally written in Latin and left with the Camaldolensian fathers who provided Rákóczi a retreat from the world of affairs after his conversion experience. The version printed here is a French translation written by Father Jourdain in the eighteenth century. The first book (1676-1703) describes Rákóczi's youth and education and ends with his escape from Austrian imprisonment on the eve of the rebellion. Books two and three (1712-17) carry the story to the moment when Rákóczi decided to leave France and to try once again to win Hungarian

independence, this time with the assistance of the sultan.

The two documents are very different both in purpose and form. The *Mémoires* offer a straightforward narrative of events and justification of Rákóczy's actions in the civil war. The *Confession* is a more personal document in which the description of events is scattered among long passages reminiscent of St. Augustine. In the last two sections, Köpeczi has mercifully excised many of these tedious, repetitious invocations of divine grace. There are extensive notes correcting errors in the text, adding exact dates, and identifying individuals mentioned only casually. Each document reveals a different side to Rákóczy's personality: the *Mémoires*, a young, charismatic national leader infused with the tribal mentality of his class and the sense that providence had chosen him to restore the traditional liberties of the Magyar nobility; the *Confession*, a Jansenist recluse deeply troubled by a sense of guilt and equally troubled by the inscrutability of a providence that permitted the Austrian Habsburgs to win the war.

Like Peter the Great, whom he tried to win over to the anti-imperial cause, Rákóczy was a "westernizer" among his own people, whose military and political backwardness he recognized clearly and reveals unsparingly in the *Mémoires*. The undisciplined incompetence of the noble officers combined with the casual looting and desertion of the ordinary men who flocked to his standards condemned his movement to military failure. Although Rákóczy's view of the military situation was incisive, his judgment of the diplomatic and political scene was naive. It was clear to him from the beginning of the rebellion that the nation would have to provide substantial resources to defend its independence, but he could not discover how to extract these resources without alienating his supporters. Habsburg diplomacy and the success of Anglo-Imperial armies in the war against France frustrated Rákóczy's hopes for foreign assistance to overcome the weakness of his own forces in Hungary.

Köpeczi has done a great service in making these documents easily accessible to Western scholars in a modern edition. One wishes for more scholarly references in the notes, for a more ample bibliography (a few secondary works in French are mentioned, nothing more), and a more usable index. The copy sent this reviewer had a substantial section of pages bound out of order.

JOHN P. SPIELMAN
Haverford College

PETER I. HIDAS. *The Metamorphosis of a Social Class in Hungary during the Reign of Young Franz Joseph* (East European Monographs, number 26.) Boulder,

Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1977. Pp. xvi, 140. \$12.00.

The purpose of this monograph is to clarify the political situation in Hungary between 1849 and 1853. In the first part of the study Peter I. Hidas describes the Hungarian policies of the liberal centralist Schwarzenberg cabinet. True to the principles of the Stadion Constitution of 1849, the liberal centralists attempted to "provide justice, security and prosperity for all classes and nationalities" (p. 83). An important agency to this end was the newly established gendarmerie, which ensured "an improved climate for the orderly modernization of Hungarian economy" (p. 14). The government aimed at economic rejuvenation of the country. The pre-1848 tariff walls between Austria and Hungary were dismantled. Hungarian agricultural products found ready markets in Austria and industrial goods became cheaper in Hungary. The government built new roads, railroads, and waterways and compensated the nobility for its losses in 1848. The result was increased prosperity. The liberal centralists even improved the school system, and "they flinched from muzzling the press" (p. 25). Their administration reached the height of its power in the fall of 1850. Thereafter, their influence in Vienna was challenged by the absolutist centralists led by Friedrich von Kübeck. Opposing progressive liberalism, the absolutist centralists received the support of powerful court circles and the Catholic Church. After the death of Schwarzenberg they succeeded in bringing liberal rule to an end by 1853.

The second half of the book deals with the impact of liberal centralist government policies on the large estate-owning aristocracy, and the *hene possessionati* gentry in Hungary. After 1849 both of these groups rejected Kossuth's program of complete independence, but they also opposed the liberal centralists. Led by the so-called Old Conservatives, they wanted to re-establish Hungary's pre-1848 constitutional position and safeguard its territorial integrity within the Habsburg monarchy. They also stood for Magyar supremacy in Hungary.

Hidas's sympathies are obviously with the liberal centralists, and he gives a very favorable account of their accomplishments. The Hungarian nobility, which after all rejected the progressive measures of the Viennese reformers, is treated with less understanding, and the author fails to make it clear exactly what he means by the nobility's "metamorphosis." It seems that instead of a metamorphosis it would be safer to talk about the nobility's temporary disorientation after 1849. The evidence does not show that the nobility gave up the great ideals of the 1848 revolution or changed its

basic political or ideological outlook. The greatest social gains of 1848, the liberation of the serfs and legal equality, were not challenged after 1849. Therefore, they received little attention in Hungary during the postrevolutionary period. Since Hungarian autonomy and constitutionalism were abolished, the nobility logically concentrated on regaining these national rights. The leadership of the Old Conservatives was temporarily accepted. This did not constitute a metamorphosis, however, only a realistic change in tactics, because after the defeat of the revolution only those who had remained loyal to the dynasty in 1848 could argue for Hungarian autonomy.

Apart from the debatable points of interpretation, Hidas's monograph should be regarded as a serious scholarly work. It is based on research in the Hungarian National Archives and shows familiarity with contemporary newspapers and a great many other primary sources and secondary works. It provides much useful information about a hitherto neglected historical period.

LASZLO DEME
New College,
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IVAN VOLGYES and NANCY VOLGYES. *The Liberated Female: Life, Work, and Sex in Socialist Hungary*. (Westview Special Studies on the Soviet Union and Eastern Europe.) Boulder, Colo.: Westview Press. 1977. Pp. xiv, 240. \$13.25.

Ivan and Nancy Volgyes set out to examine "the Hungarian experiment to liberate women from servitude," though the emancipation of women was far from the first priority of the regimes that came to power in Eastern Europe after World War II. Their book offers few surprises, but it does provide valuable details on the problems of Hungarian women in employment, in the household (the "second shift" that men escape), and on the "third shift," that is, sexual relations. It outlines the wide-ranging social policies of the government and makes clear the persistence and pervasiveness of patriarchal cultural values in socialist society.

Legally, of course, women in Hungary are entitled full political, social, and economic equality. They work in every field: industry, construction, agriculture, transport, telecommunications, commerce, the service sector, the professions, and public affairs. When the authors investigated the structure of employment they found, of course, that women are not equal in the workplace; certain jobs continue to be regarded as "men's work" and others "women's work." Women tend to be employed in jobs with less prestige and less pay, even though automation is pushing women out of

traditional unskilled sectors. There is little room at the top for women because childbearing interrupts their career patterns and because they lack the qualifications as well as the encouragement to continue their education.

Women shoulder the household burdens of shopping, cooking, cleaning, and childcare insisted upon by traditionalist husbands, and they have little time or energy for the public versus the domestic sphere. To be sure, the state has attempted to alleviate the triple burden of outside work, housekeeping, and childrearing—but within the context of a precariously low population growth. While contraception and abortion are easily available, the state also provides generous maternity incentives. It has opted, in effect, not to expand public child care but is paying women to return to the home and raise children. The "Young Mothers' Leave" plan allows women to stay home with their children up to the age of three, paying them six hundred forints a month and guaranteeing them a return to their jobs. For a mother of three, this can last up to nine years.

The authors suggest that, if Hungarian women are to achieve greater equality, they need to receive more technical training; and supermarkets and more prepared packaged foods as well as kindergartens and laundries need to be provided. They recognize, however, that "if equality is the true aim of the present regime, it should begin a thorough attempt to alter the present role-socialization of Hungarian children" (p. 207).

Using a breezy, journalistic, anecdotal style somewhat inappropriate in an obscure hardcover edition, the authors do convey a concrete and colorful picture of the everyday life, dilemmas, problems, and pleasures of a wide range of women—professionals, prostitutes, singles, marrieds, unskilled laborers, and peasants. They derived their information from repeated visits to Hungary, personal and random interviews, discussions with government officials, statistical reports, and popular literature, including cartoons, some of which appear in the book. The authors do not mention any women's movement in Hungary, but surely a socialist country contains a variety of women's organizations whose leaders might have been interviewed. There is no real scholarly apparatus, and there are no references either to any comparative literature about women in other socialist countries or to any theoretical critical works on feminism and socialism. The authors do not really probe deep and fundamental questions as did Hilda Scott in *Does Socialism Liberate Women?* (1974), a case study of Czechoslovakia. Finally, the handling of the relationship between the sexes in socialist Hungary is smug, superficial, and sentimental—describing women as "charming" and

"womanly" and giving exaggerated attention to the phenomenon of prostitution.

INGRUN LAFLEUR
William James College,
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A. S. MYL'NIKOV. *Epokha prosveshcheniia v cheshkikh zemliakh: Ideologiia, natsional'noe samosoznanie, kul'tura* [The Epoch of the Enlightenment in the Czech Lands: Ideology, National Self-Consciousness, Culture]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 198. 1 r. 30 k.

During the eighteenth century the Czechs, who were then one of the subject nationalities of the Austrian Empire, reached their nadir as a nation. Their governing class had been almost completely Germanized. The Czech language ceased to be the medium of high culture; none but the cooks, the stable boys, and the peasants would stoop to employ it as a means of communication. The Czechs had become a nonnation, an invisible people. Little wonder Czech historians later dubbed this period *temno* ("darkness"). Yet, amid this retrogression, there appeared the first signs of a national revival—a movement nourished by the European Enlightenment. It is to this period and to this theme that A. S. Myl'nikov addresses himself. To be more specific, he explores the emergence of Czech national consciousness in the context of the European Enlightenment and analyzes the Enlightenment ideology of the first Czech national "awakeners."

The treatment of the topic tends to be pedestrian; at the same time, the style is a bit ponderous and the message is not always clear. Marx, Engels, and Lenin shine, as authorities quoted in the opening and closing sections of the chapters. For a change of pace, the first chapter opens with a quotation not from any of the Marxist classics but from the late Czechoslovak Communist President Gottwald—a gesture presumably designed to bolster the ego of Czech Marxists. The core of the volume is section three, which examines the views of the Czech awakeners on the social and political organization of society. Here the interest of the reader will be arrested by two chapters, entitled "The Struggle for Freedom of Conscience" and "The Struggle for Freedom of the Mind." It is rare for a Soviet historian to display so boldly before the reader terms connoting intellectual freedom. How little such concepts count in the Soviet Union may be seen from the fact that the celebrated *Soviet Historical Encyclopedia* (1961–76) has no entry for "freedom." It is true that Myl'nikov tends to present the struggle for freedom as a fight against the Church and thereby helps the Marxist cause. Nev-

ertheless, he does not belabor the anti-Church angle unduly; and it is refreshing to see such a focus on freedom in a Soviet work, even when it is in the danger of being smothered by quotations from the Marxist classics.

The author regards the intelligentsia as the prime mover of the early stages of the Czech revival, in line with established opinion. He appears, however, not to do justice to the Catholic clergy as a group, although he does credit individual clergymen with having contributed substantially to the Czech awakening. It seems that Marxist flesh continues to creep, as it has done for decades, at the sight of the clerical order doing anybody any good. One shortcoming of the book is that Myl'nikov presents almost no quantitative data, even in cases where this would be clearly warranted, such as in his references to literacy and the strength of the various social classes. On the other hand, the work has one important merit: in constructing the profile of the awakeners, the author has drawn on some rarely used contemporary periodical sources, such as published collections of sermons, and thus has enlarged our understanding of the intellectual climate.

The task Myl'nikov set for himself was to analyze the Czech Enlightenment. In his concluding reflections, he expresses the hope that other historians will soon go beyond narrow national boundaries and will compare the Czech experience with that of other nations. This will enable us to see what was routine and what was unique about the Czech Enlightenment. I am sure any reviewer will be happy to second this proposal.

STANLEY Z. PECH
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JOSEF KALVODA. *Czechoslovakia's Role in Soviet Strategy*. Washington, D.C.: University Press of America. 1978. Pp. ix, 381. Paper \$8.75.

The title of this study is somewhat misleading. The book does not deal so much with Soviet strategy in Czechoslovakia as with Czechoslovak policies and attitudes toward Russia and the Soviet Union from the First World War to 1968—the year of the "Prague Spring" and the Soviet reoccupation of Czechoslovakia.

When, after the outbreak of World War I, Thomas G. Masaryk made the fateful decision to break with the Austrian Empire and seek Czechoslovak independence, he took it for granted that this could be achieved only with Russian military assistance. He also assumed that after the war Russian support would be essential for the maintenance of Czechoslovak independence. The war, however, took a different course from what he and

most of his contemporaries expected. Russia was defeated by Germany and plunged into revolution; Germany was defeated by the Western Allies; and Austria broke up from within, leaving a power vacuum in East Central Europe. While Masaryk was still abroad, the Czech political leaders in Prague established the Czechoslovak state in a series of deft political and diplomatic moves, virtually without firing a shot. The Allied powers, which had encouraged the Czechoslovak movement for independence during the war, unhesitatingly confirmed it at the Paris Peace Conference.

Czechoslovakia's strategic position was inherently precarious. Of its five neighbors—Germany, Austria, Hungary, Rumania, and Poland—all but Rumania were unfriendly. The original hope of most Czech and Slovak leaders to secure Russian help was disappointed. The Soviet government, which survived the Russian Civil War and Allied intervention, was unfriendly to Czechoslovakia. Even if the Soviet regime should fall, as Karel Kramář and other political leaders continued to hope would happen, Russia would not be in a good position to help Czechoslovakia, because, contrary to their original assumption, the two countries were not contiguous. Therefore, Foreign Minister (after 1935, President) Edvard Beneš oriented Czechoslovakia's foreign policy toward the Western powers and the League of Nations. In 1924 he concluded a treaty of alliance with France, which was the mainstay of his system.

So long as the Western powers dominated European politics, Czechoslovakia appeared to be safe from all danger. In the 1930s, when Germany effected a military comeback and began to exert a strong influence in international affairs again, Czechoslovakia felt threatened. In 1935 Beneš followed the conclusion of the Franco-Soviet alliance by immediately concluding a Czechoslovak alliance with the Soviet Union, which was to become operative only when France honored its commitment to defend Czechoslovakia. He was not able to secure an alliance with Britain, but Britain committed itself to defend France in the event of a war with Germany. On paper, Beneš's alliance system appeared perfect. A German attack on Czechoslovakia would automatically bring to its rescue France, the Soviet Union, and even Britain. When the system was put to a test during the Munich crisis in 1938, however, it completely failed to function. The key position in the crisis was held by Britain, which refused to go to war over Czechoslovakia. France refused to act without Britain and the Soviet Union without France. Beneš refused "to lead the nation to the slaughterhouse in an isolated war" and surrendered to Hitler's demands. In March 1939, however, when Hitler liq-

uidated rump Czechoslovakia, Beneš, who had gone into exile in London, organized a movement for his country's restoration.

During the course of World War II the Allied powers recognized Czechoslovakia's legal existence and the right of Beneš's London government to represent its interests. Nevertheless, because of the role of France and Britain in the Munich crisis, Beneš remained distrustful. In December 1943, against British advice but with Roosevelt's indifferent blessings, he went to Moscow and concluded a treaty of alliance with the Soviet Union, which he hoped would not only insure Soviet assistance in the war but also Soviet noninterference in Czechoslovak internal affairs after the war. In the latter expectation, however, he was disappointed. In February 1948 the Czechoslovak Communists threatened to seize power by force. Once again, as he had done ten years before when he submitted to Hitler's exactions, Beneš yielded to the Communist demands without a fight. The Communists took power in a bloodless coup.

Beneš formally belonged to the Czech political Left, but concerned himself little with internal politics. Josef Kalvoda, a Czech political exile in the United States, approaches his subject from the point of view of the Czech political Right. He sees few virtues in Beneš's policies. He believes that the Soviet-Czechoslovak treaty of 1943 opened the way to Soviet control and the communization not only of Czechoslovakia but of all the countries of East Central Europe. In this belief he exaggerates the efficacy of diplomacy and importance of treaties as much as Beneš did. Not all international questions are decided by negotiation and treaties; some of them are settled by force. The Soviets have established their domination over Czechoslovakia and other countries of East Central Europe, not as a result of this or that treaty but because of the fact that the Soviet army conquered the area from Germany in World War II and the Western powers chose not to dispute the Soviet Union's control. At the time of the conclusion of the Soviet-Czechoslovak treaty, when some members of Beneš's London government expressed doubts about the wisdom of placing Czechoslovakia's fate in Soviet hands, Foreign Minister Jan Masaryk replied, "But boys, we are not on Eisenhower's map"—meaning that the Western armies were leaving the task of liberating Czechoslovakia to the Soviet army. Beneš might have preferred to ride into Prague in the baggage van of the Anglo-American armies instead of that of the Red Army, but he knew that the British and Americans were not going there.

This book contains much new material, which students of East Central European affairs will read with great interest. It fails completely, however, to

present the Czechoslovak problem in the context of great power relations.

VICTOR S. MAMATEY
University of Georgia

RAYMOND PEARSON. *The Russian Moderates and the Crisis of Tsarism, 1914-1917*. New York: Barnes and Noble. 1977. Pp. x. 208. \$22.50.

The title of this book is somewhat misleading. Although purporting to examine the fate of Russian liberals during the Great War, in reality Raymond Pearson convincingly demonstrates the searing impact of prewar events—and especially the Vyborg Manifesto of 1906—upon the Kadet Party and its political allies in the State Duma during the final years of the imperial regime. At Vyborg, it will be recalled, the Kadets of the First Duma assembled to urge a nationwide protest movement over the government's dissolution of the legislature. Popular response to the call was insignificant, and this, in Pearson's words, "prevented its [the Kadet Party] ever recovering confidence in democracy or retrieving a healthy relationship with the country which it claimed to represent" (p. 37). Accordingly, in 1915, during the public clamor for a new ministry to successfully guide the nation at war, the Kadets drew back from the allure of direct action, the memories of Vyborg too fresh to overcome. The result was a tacit alliance between the Duma and a reactionary officialdom which, once the latter collapsed, deprived the Provisional Government, composed for the most part of ex-Duma leaders, of authority to enforce its will over the soldiers and restless workers in 1917.

Pearson's book is based on solid research, much of it in Soviet archives previously barred to Western scholars. Particularly interesting is the discussion in the introduction of the usefulness of the Okhrana file, a prime source since Duma liberals and their counterparts in the wartime public organizations (the Union of Towns and Union of Zemstvos) encountered constant surveillance from the secret police. As Pearson notes (and as my own work on the public organizations confirms), the rank-and-file police reports accurately, and sometimes prophetically, dissected the worsening condition of the body politic. Indeed one suspects that they were passed over by bureaucratic higher-ups to their distinct peril and eventual oblivion.

One noteworthy theme to emerge from this study is the decisive role of the last tsar. At critical junctures, including the prorogation of the Duma as well as his assumption of military command in August 1915, Nicholas II acted alone, against the advice of his ministers. The portrait of the tsar in

control of his faculties, though misguided in his policies, serves as a necessary antidote to the generally held view of a weak-willed, passive ruler.

Pearson's book is not without its shortcomings. The prose is dry and flat. Except possibly for Miliukov, the main actors in the drama do not assume a lifelike pose. One searches in vain for the flesh and blood behind the men who dominated public life on the eve of Europe's most explosive revolution of this century. Finally, the author failed to consult the published sources for the public organizations, an unfortunate omission since he describes their political profile.

None of these lapses, however, neutralizes the book's contribution to our understanding of one of the principal causes of the February Revolution: the inability of men of liberal vision to devise acceptable alternatives to the extremes of violent revolution or counterrevolutionary violence.

WILLIAM GLEASON
University of Louisville

V. A. EMETS. *Ocherki vneshnei politiki Rossii v period pervoi mirovoi voyny: Vzaïmootnosheniia Rossii s soïuznikami po voprosam vedeniia voyny* [Essays on Russian Foreign Policy during the First World War: Russian Relations with the Allies on Questions of Conducting the War]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 365. 1 r. 36 k.

This is a disappointing book. Much of it is composed of parts of three essays first published by V. A. Emets in *Istoricheskie zapiski* ten and even twenty years ago. The remainder gives the appearance of having been written at the same time.

Emets's approach to the wartime relations of the Entente is unimaginative, and his use of evidence (especially given the archival resources at his disposal) less than compelling. He offers neither a new interpretation nor fresh evidence in support of established views. Thus, he uncritically accepts every claim put forward by *Stavka* and, strangely for a Soviet historian, seems prepared to endorse the views of General M. V. Alekseev as the ultimate authority on every question. As a result he does not analyze the decision-making structure of the tsarist government, the conflicts within that structure, the relationship of the political and military crises which emerged during the war, or any other question of significance. Instead, he accepts the argument put about by *Stavka* that Russian defeat was caused by want of Allied cooperation, a lack of shell, and the generally poor supply of the army. He completely ignores the political substance of these charges and consequently fails to examine the multitiered crisis of authority within Russia and its impact on the broader conflict

among the Allies. Predictably, neither the success of the Brusilov offensive nor the improved supply of the army in 1916 are analyzed.

Emets tells us that the world war illustrated the "bankruptcy of tsarism," that this bankruptcy was "a lawful consequence of both internal and external causes," and that these causes stemmed primarily from the "economic backwardness and unpreparedness" of Russia. "Imperialist contradictions" crippled the Allied war effort and prevented the Entente from launching a coordinated offensive which would have brought about the swift defeat of Germany. Peculiarly, however, some states were more imperialist than others. France, Britain, and Italy all pursued "narrow" imperialist objectives while Russia, we should not be surprised to learn, exhibited a much "broader vision," seeking repeatedly to interest the Western powers in a coordinated strategy and a more efficient use of resources. Allied failure to adopt *Stavka* policy, Emets concludes, made defeat "inevitable" and hastened the coming of the revolution in Russia.

There is little here to interest either the specialist or the more general reader.

RICHARD K. DEBO
Simon Fraser University

N. S. ZAKHAROV. *Sovety Srednego Povolzh'ia v period bor'by za diktatury proletariata* [The Soviets of the Middle Volga During the Struggle for the Dictatorship of the Proletariat]. Kazan': Izdatel'stvo Kazanskogo universiteta. 1977. Pp. 276. 2 r. 39 k.

Until recently, Western historians have ignored the Russian Revolution in the provinces or have viewed it as a reflection of events in the capitals. Even though interest in provincial history has grown in the West, historians conducting such studies are limited by lack of adequate documentation—not only because archival materials are restricted for the most part, but also because lacunae exist in the pertinent sources. For these reasons, provincial studies by Soviet scholars are important as factual accounts and as bibliographic aids.

N. S. Zakharov, author of the volume under review, teaches at Kazan University. He has written two previous studies of revolutionary developments in the Middle Volga area: (with I. A. Emel'ianova) *Sovety Srednego Povolzh'ia v period podgotovki Oktiabr'skoi revoliutsii* [Soviets of the Middle Volga During Preparation of the October Revolution] (1967); and *Oktiabr'skaia revoliutsiia i sovetskoe stroitel'stvo v Srednem Povolzh'e* [The October Revolution and Soviet Building on the Middle Volga] (1970). Together with E. I. Medvedev, V. Morozov, and I. M. Ionenko, he ranks as a leading Soviet specialist on the revolution in this geographic region.

Intended for a narrow scholarly audience, this monograph focuses on soviets in the Middle Volga provincial capitals of Kazan, Penza, Samara, and Simbirsk. Attention is also given to *uezd* soviets in these provinces, and, when events there fit his arguments, to Saratov province on the Lower Volga. As the title suggests, emphasis is on the soviets as institutions, rather than on the revolutionary movement. With the exception of the last chapter, which describes the structure of a typical soviet, material is arranged chronologically, according to usual Soviet periodization. Chapter seven is probably the best, especially the author's survey of the soviets' administration of justice in the fall of 1917 (pp. 194–206). Apart from this, there is little that will surprise the knowledgeable reader. For most, the facts might be new, but not the interpretations.

If Zakharov's work is at all representative of current Soviet historiography, it seems that treatment of the SRs, particularly the Left SRs, has hardened since the 1960s. Indeed, the most disappointing feature of the book is its failure to acknowledge the role of the SR Party and, to a lesser extent, the fortunes of local Menshevik organizations. The political mood of workers and soldiers in Kazan had swung far to the left by the fall of 1917, largely due to the popularity of SR Maximalists. Zakharov dismisses their role perfunctorily. Likewise, Samara in 1918 became the center of an anti-Bolshevik SR government. Yet one would be hard put to trace the roots of this potential anti-Soviet movement in the author's treatment of the political climate in the city in 1917.

Because of Zakharov's reluctance to acknowledge the role of the SR Party and to assess its agrarian program, he has given little attention to the few peasant soviets that existed before October. One could also take issue with Zakharov's limited account of the Muslim population of Kazan, of his cavalier statements regarding Bolshevik popularity, and of his neglect of the anarchists.

In sum, although the monograph is based on extensive use of archival material, the periodic press, memoir literature, and Soviet secondary accounts, the final result is disappointing. In view of the author's rigidly orthodox interpretations, it will be up to Western scholars to provide a more objective study of the area. Kazan and Samara, if not the other cities as well, merit such attention.

DONALD J. RALEIGH
University of Hawaii

S. S. KHESIN. *Moriaki v bor'be za Sovetskuiu vlast'* [Sailors in the Struggle for Soviet Power]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 174. 65 k.

NORMAN E. SAUL. *Sailors in Revolt: The Russian Baltic Fleet in 1917*. Lawrence: Regents Press of Kansas. 1978. Pp. xiii, 312. \$12.50.

The sailors of the Russian Navy played a role in the Bolshevik victory out of proportion to their numbers; the Bolsheviks found as many armed supporters in the navy of less than two hundred thousand as in the army of over seven million. Following the October Revolution the importance of the sailors further increased: in that time of confusion a few reasonably well-organized detachments repeatedly saved the new regime from collapse. Two new books, one Soviet and one American, attempt to describe the contribution of the sailors to the victory and consolidation of Soviet power.

The author of the Soviet book is S. S. Khesin, who has written several books and articles on this subject. His work, evidently intended as a contribution to the sixtieth anniversary celebration of the revolution, contains neither new material nor a new interpretation. Although it was published under the auspices of the Soviet Academy of Sciences, it appeared as part of a "scientific-popular series," published in sixty thousand copies. Clearly it is aimed at the general reader.

Reading Khesin's book is a depressing experience; it exemplifies the worst in Soviet historiography. Khesin has little respect for the truth. He repeats clichés of Soviet propaganda, for example, the charge that the Allies from the very first were coordinating the moves of "counter-revolutionaries" against the victorious Soviet regime. In describing the revolutionary movement among the sailors he hardly finds occasion to mention that in 1917 Russia was engaged in fighting a war, and that the navy had, in fact, just fought its largest engagement at Moon Sound in September of that year. Not surprisingly, he fails to mention Trotsky, the first commissar for foreign affairs, but gives credit to sailor Markin for publishing the secret treaties of the previous governments. More significant and discouraging is that Khesin fails to mention F. Raskolnikov, one of the most important Bolshevik leaders of the Baltic Fleet and after November 1917, a deputy commissar of the navy. Serving as Soviet ambassador to Bulgaria at the time of the purges, Raskolnikov defected rather than obey an order to return, which would have meant certain death. In the early 1960s he was rehabilitated, but Khesin, writing in 1977, mentions Stalin approvingly in his book while condemning the tyrant's victim to oblivion once again.

Perhaps it is Khesin's style that is most irritating. He evidently sees no need to change the tone characteristic of Stalinist historiography. A single example must suffice. The author describes how,

in January 1918, the Ukrainian Rada, which was almost in a state of war with the Bolsheviks, forbade the sailors of the Black Sea Fleet to maintain contacts with the Soviet regime. Then follows a perfectly typical sentence: "This insolent demand of the Rada created profound indignation among the Black Sea sailors" (p. 144). And so on.

His logic is not much better than his prose. In the course of ten brief pages he makes the following three incompatible assertions: (1) only the Bolsheviks stood for revolution, and the Mensheviks and Socialist Revolutionaries betrayed the interests of the people; (2) "the entire fleet came quickly and decisively to the support of the popular uprising" in February 1917 (p. 21); and (3) at the time of the February Revolution the Black Sea sailors were almost completely under the influence of the Socialist Revolutionary and Menshevik compromisers.

Norman Saul's focus is narrower than Khesin's. The American historian limits himself to describing the most important and most revolutionary fleet in 1917, the Baltic Fleet. This is unfortunate because it is likely that we could learn much by comparing the circumstances in which the Bolsheviks were most successful with the situation prevailing in the other fleets.

Saul's research is impressive. He worked in several archives, the most important being the Helsinki Archives and the London Public Record Office, and he accumulated a considerable amount of new material. He is also familiar with the writings of Soviet historians. He uses his sources judiciously, and when he ventures an opinion, it is always sound and well supported.

The author has attempted to place his subject matter in a larger context. Before approaching the revolutionary events of 1917, he concisely describes the politics of Russian naval construction, the fighting ability of the navy, and the revolutionary tradition among the sailors. Unlike Khesin, he is well aware that in 1917 a war was going on. Indeed, he describes the battle at Moon Sound in some of the best pages of the book. Nor can it be said that Saul neglects the revolutionary background against which the movement of the sailors took place. Yet the book is ultimately disappointing in spite of these virtues. Saul has no well-developed view of the Russian Revolution, and, under the circumstances, he cannot tell us how the sailors fitted in and how important they were. It is a book without arguments. Since the author provides no larger framework, we do not appreciate the significance of the details. We just go from demonstration to demonstration, from meeting to meeting, from crisis to crisis.

PETER KENEZ
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S. FREDERICK STARR. *Melnikov: Solo Architect in a Mass Society*. Princeton: Princeton University Press. 1978. Pp. xvii, 276. \$25.00.

This definitive and lavishly illustrated study of Soviet Russia's leading modernist architect rejuvenates a reputation. Until his political demise in 1937, Konstantin Melnikov was Soviet Russia's leading architect. Grandson of a serf, he had risen in a single generation to international fame and such fortune as was possible in the Russia of the 1920s. Doomed to live in Moscow in inner exile until his death in 1974, Melnikov managed to create a number of successful buildings geared to the rational visions of a new, revolutionary society: the 1925 Soviet exposition pavilion in Paris, the double interlocking cylinder structure of his own Moscow house, and an Intourist parking garage for buses. Utilizing interviews with Melnikov himself and access to his private archive, S. Frederick Starr has lovingly reconstructed the life and work of a forgotten visionary.

Melnikov's career gives a vivid picture of many elements in early Soviet culture: its utopian dreaming (only twenty of Melnikov's eighty projects were actually built); the continuity between the romantic classicism of late imperial Russian architecture and 1920s constructivism; and the extensive links between Soviet Russian and European modernist architecture after the revolution. But Starr correctly identifies a more profound and subtle element in that culture—a belief in a victory over death through revolution. Raised in an orthodox Christian culture, Melnikov believed in a form of resurrection derived from the notion that death is simply a form of sleep. Melnikov's Lenin sarcophagus therefore assumed that the dead leader could some day be awakened like the sleeping princess by a signal from the Russian people; his designs for Paris and Moscow parking garages also ascribed to turned-off automobiles a quality of sleep or death; his own bedroom was a kind of tomb for nightly rejuvenation; and a "Sonata of Sleep" pavilion in his utopian Green City would have provided anxious urbanites with an escape from city cares through sleep therapy. Throughout this book one is aware that rationalist construction is informed by irrational mysticism.

As a study of an important and unknown figure, this book is a major contribution to the history of architecture and of early Soviet culture. But the author's erudition and imagination continually elevate it from public and private biography to broadly conceived cultural, social, and political history. Melnikov's career thus helps us understand that irrational yearnings were as important to Russian revolutionary culture as its public rationalist visage.

ROBERT C. WILLIAMS
Washington University

ROBERT C. WILLIAMS. *Artists in Revolution: Portraits of the Russian Avant-garde, 1905-1925*. Bloomington: Indiana University Press. 1978. Pp. x, 242. \$15.00.

In recent years, the study of the Russian avant-garde in the first quarter of the twentieth century has drawn considerable attention. This work presents a series of portraits of significant leaders of Russian culture in that highly creative period preceding the advent of "official culture" in the early 1930s. In constructing his collective portrait, Robert C. Williams has gone beyond the realm of painting and decorative arts to develop a wide-ranging study of the Russian intelligentsia, including figures like Lunacharsky, Meyerhold, Mayakovsky, Tatlin, Malevich, Eisenstein, Dobuzhinsky, and Moor. The dominant concept uniting these sometimes diverse individuals is, according to Williams, their concern with death and artistic and revolutionary immortality. Each artist was obsessed to some extent with death and, therefore, sought a means of attaining a new vision of immortality.

Perhaps the most interesting section of Williams's work deals with Kazimir Malevich's interest in theosophy and the search for the fourth dimension. Malevich's involvement with the theories of theosophy through the little known works of Charles Hinton, Claude Bragdon, and Peter Uspensky marked a turning point in the artist's career. Uspensky's belief in a fourth dimension, which suggested the existence of a higher realm of existence, became a dominant influence for Malevich during 1915 and subsequently provided the impetus for the artist to move from futurism to suprematism. For Malevich, the dynamic new forms were the square and cube, with the square representing man and the cube representing a higher dimension. These shapes, in turn, became symbols of the fourth dimension as expressed through suprematism.

In developing the threads of the immortality theme, Williams emphasizes several important points. First, although born as a loose movement influenced by Western ideas, the avant-garde may be seen as a genuine reaction against Westernism and another statement of the uniqueness of Russian spirituality over inferior Western concepts. Secondly, the avant-garde's search for immortality was part of a mid-life psychological crisis of all the artists, the majority of whom were born in the 1880s. Thirdly, the desire of the avant-garde for a means of attaining immortality was in essence a new form of religion that grew in Russia from artistic sources after the initial attacks on organized religions by the secular doctrines of the new Soviet state. Collectively, all of these conclusions help to tie together the diverse themes of this group. Williams's most interesting assertion, how-

ever, is that the pursuit of the theme of immortality by these artists helped to prepare the way for Stalinism by establishing the efficacy of personal death for the revolution as a means of assuring individual immortality.

Williams's work is an exceptionally useful addition to the historiography of the avant-garde. Its main fault lies in its broad conception. The theme of death and immortality unifies the study, but one is left searching for more biographical information on each figure. In addition, a monographic interpretation of this period is frustrating when the source materials themselves are so rich and able to convey the artists' theoretical conceptions. For example, the manifests in *The Documents of 20th Century Art* series, edited by John Bowlt and Stephen Bann, provide an exceptionally lively set of readings for establishing a more complete statement on the avant-garde.

STEPHEN C. FEINSTEIN
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River Falls

D. F. USTINOV *et al.* *Istoriia vtoroi mirovoi voiny, 1939–1945. Tom 8, Krushenie oboronitel'noi strategii fashistskogo bloka* [History of the Second World War, 1939–45. Volume 8, The Downfall of the Defensive Strategy of the Fascist Bloc]. Moscow: Voenizdat, 1977. Pp. 534. 2 r. 80 k.

In this volume covering the period from the end of 1943 to the end of May 1945, Soviet historians continue their offensive against the bourgeois falsifiers of the history of World War II. In particular, the lie is given to American historians who spread the falsehood that the economic and human resources of the USSR were exhausted in this period and who "incredibly exaggerate the significance of lend-lease shipments" (p. 8). To refute these fictions, considerable attention is devoted to the Soviet war economy and its "victory" over the German economy in the production of military weapons and equipment. While admitting that the lend-lease shipments had a positive influence on the Soviet struggle with Germany, the authors conclude that "it is generally known that these shipments comprised an insignificant share of Soviet production and did not play an important role" (p. 387).

One can only surmise that even such critical observers of U.S.-Soviet cooperation during World War II as Major General John R. Deane, author of *Strange Alliance*, would be disappointed at this evaluation of the approximately eleven billion dollars in U.S. aid given to the USSR during the war. Among the items Deane cites are 427,284 trucks. He also reports that on a visit to the Belorussian Front in July 1944 "we encountered American

trucks everywhere" (p. 93). Deane does not claim that United States' supplies won the war but he does suggest that "they must have been comforting to the Russians" (p. 95). Current Soviet historiography of World War II seems to find this aid more of an embarrassment to be explained away than a comfort.

Whatever view one holds as to the importance of American aid to the Soviet war effort, the achievements of the Soviet war economy should be recognized and deserve study. Forced to evacuate people and industrial equipment to the east under unbelievably difficult circumstances in 1941 and 1942, by the middle of 1944 the Soviets were producing more tanks, assault guns, self-propelled guns, and aircraft than were the Germans. What is not clear is the organizational mechanism through which these achievements were accomplished. In general, they have been ascribed to the superiority of a socialist economy. Somewhat less generally we are told of decisions of the State Defense Committee (GKO), chaired by Stalin, and its plenipotentiaries who were sent to problem areas with authority to resolve local difficulties. In 1974 the existence of an "Operations Bureau of the GKO" was revealed. Formed in December 1942, the Operations Bureau apparently functioned as an economic general staff controlling all of those industries connected with war production. It has not been revealed who was chief of this economic general staff—perhaps it was N. A. Vaznesenskii, who, like many of the Soviet wartime military leaders, ran afoul of Stalin in the postwar period. Perhaps this question will be clarified in subsequent volumes. The first eight volumes, unfortunately, have not been notable for efforts to break new ground in clarifying such murky areas of Soviet World War II history.

WILLIAM J. SPAHR
Alexandria, Virginia

A. M. SAMSONOV *et al.* *Sovetskii soiuz v gody Velikoi Otechestvennoi voiny, 1941–1945* [The Soviet Union During the Great Fatherland War, 1941–45]. Moscow: Izdatel'stvo "Nauka," 1976. Pp. 727. 4 r. 23 k.

This one-volume history of the Great Patriotic War is intended to replace *Velikaia Otechestvennaia Voina Sovetskogo Soiuza, 1941–1945: Kratkaia istoriia* published in 1967. The latter volume was a condensation of the six-volume history of the Great Patriotic War published in 1962. Although the condensed volume was not as outspoken as the complete set, there were still some hints of criticism about the performance of Soviet military and political leadership during the war.

The volume under review carries no such distinction and is a good example of Soviet historio-

graphy under the present regime. The emphasis here is on the "positive"; the authors ignore unpleasant facts such as the early massive setbacks despite the possibility (raised persuasively by General P. G. Grigorenko) that in 1941 the Red Army was superior to the Wehrmacht. Although the historical meeting following the maneuvers of December 1940 is mentioned, no reason is given for the unpreparedness of the Red Army, nor for Stalin's decision to ignore both the warnings of his agents and the continuing concentration of German forces on the western frontiers. There is no criticism of Stalin; the early scapegoats, such as Kulik, Mekhlis, and Beria, are allowed to rest in peace. The same principle is also applied to Khrushchev, once lionized and later damned. Western powers come in for their usual share of criticism. The book's treatment of the greatest armed struggle in history is no more than a chronological listing of various events sprinkled with the usual Soviet propaganda. It is history in its most antiseptic form, useful only as an example of the sad state of modern historiography in the USSR. Based almost entirely on secondary sources, the book has an extensive bibliography and good maps but no index.

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TÖNU PARMING and ELMAR JÄRVESOO, editors. *A Case Study of a Soviet Republic: The Estonian SSR*. Foreword by EDWARD ALLWORTH. (Westview Special Studies on the Soviet Union and Eastern Europe.) Boulder, Colo.: Westview Press. 1978. Pp. xxiii, 432. \$22.00.

Arthur Koestler, writing in the first volume of his autobiography, *Arrow in the Blue*, recalls a comment made to him in 1931 by a Russian radio operator: "Esthonia? It is not an interesting country. In Russia we call it 'the potato-republic.'" Unjust then, this remark is even less applicable now to the least populous but perhaps most intriguing of the constituent units of the multiethnic Soviet Union, the Estonian Republic. *A Case Study of a Soviet Republic: The Estonian SSR*, edited by two Estonian-born American scholars, Tõnu Parming and Elmar Järvesoo, demonstrates just how misleading any monistic view of the Soviet republics may be.

The fourteen contributors to this outstanding and beautifully written compendium address themselves to the "imperfect interaction and partial disconnection between the various eponymous nationalities and the Soviet socialist republics (SSRs) named for them" (p. ix). Arranged under

four broad subject headings (Population and Political Processes, Economic and Societal Processes, Aspects of Culture, and Higher Education, Research, and Science), the articles offer a comprehensive view of what seems to be a deviant case—the dominance within the Estonian SSR of the Estonian nationality.

The basic contention of this volume is that, within the universe of Soviet republics, the Estonian SSR is different. In their introduction, Parming and Järvesoo suggest several factors that help to explain this difference, stressing such variables as Estonia's pre-Soviet history, a strong sense of cultural identity among native Estonians, and, significantly, the tolerant attitude of Moscow. One important measure of the special place occupied by Estonia and the Estonians, discussed in the essay by Pennar, is the extent to which the Estonian Communist chief Kabin has managed to "nationalize" the republic's party elite, in membership if not in policy, while at the same time securing for himself a respected voice in central party councils. The conclusion that must be drawn from the studies in this volume is that it makes considerable difference to the life of the Estonian SSR that a majority of its inhabitants—and apparently of its social, intellectual, and political elite—are Estonians, not Russians or Ukrainians, as is the case, for example, in Kazakhstan.

The major difficulty with the book derives from neither its choice of subject nor its organization, but rather from the type of treatment it affords individual topics and the subject as a whole. Edward Allworth's elegant foreword and the extremely detailed analysis of certain topics—population processes, literature, and scientific research, in particular—suggest that the book is primarily addressed to those already reasonably knowledgeable about the problem of nationality in the Soviet context and about developments in Estonia. The absence of a broad interpretive history of main developments in the Estonian SSR since 1940 and the lack of an integrative conclusion paralleling the excellent introduction leave the nonspecialist historian, in particular, without a useful framework within which to assimilate and assess the vast amount of significant topical detail presented in this volume.

Despite flaws, *A Case Study of a Soviet Republic* makes a signal contribution to a more sophisticated understanding of Soviet politics and society at the subnational level. It argues indirectly, but strongly, that whatever close relationship may exist between a nationality and the SSR named for it depends primarily on special historical and social factors. This ambiguous status of SSRs and other "ethnic" territorial units supplies much of the political dynamics to subnational Soviet life. It is

unfortunate, however, that the role of the relationship between ethnicity and social stratification and of intra-Communist Party processes in these dynamics must await the generation of, and access to, vital relevant data.

We can be grateful to the authors and editors of this volume for adding immeasurably to our knowledge of and sensitivity toward the complexities of life in at least one Soviet socialist republic. They are to be congratulated for tackling this complicated and at times emotion-laden issue with meticulous scholarship and objectivity. Parming, Järvesoo, and their collaborators have laid a necessary, indeed indispensable, foundation for a rigorous, comprehensive study of the place of the SSRs in the Soviet scheme. We may hope that others will continue to build on this foundation.

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DOMINIQUE LECOURT. *Proletarian Science? The Case of Lysenko*. With an introduction by LOUIS ALTHUSSER. Translated by BEN BREWSTER. New York: Schocken Books. 1977. Pp. 165. \$11.50.

Dominique Lecourt's book, as the author puts it, relies "on essentially the same documents" as have been used by Zhores Medvedev, David Joravsky, and Loren Graham, historians of Soviet science who have analyzed the Lysenko affair. Frankly the author seems to have had no other choice, for he never reveals any knowledge of Russian in the course of the book and consequently has no first-hand acquaintance with primary sources. Nevertheless, even with this severe limitation, the reading is worthwhile for several reasons.

First and foremost, the author discusses Lysenko's history in terms of Western Marxian thought. Using his Marxist background (he goes to extremes in guarding his Marxist chastity by using more neutral phrases such as "Lysenkoism," or "in the case of Lysenko," in place of the term "Lysenko affair," which he considers a symbol of the reactionary, idealistic approach to history), Lecourt sets the task of outlining the features of Marxist ideology that contributed most to Lysenko's success. This is a very challenging goal indeed.

In the first chapter the author analyzes the initial reaction of French Marxists to the rise of Lysenko in the USSR in 1948. The reaction was one of unanimous and enthusiastic support, with articles in *Les Lettres Françaises*, the formation of the French Association of the Friends of Michurin, and the publication of a kind of theoretical manifesto, *Science bourgeoise et science prolétarienne*. The material used in this chapter is relatively unknown

and very interesting. Using this material as a starting point, Lecourt proceeds to answer one of the classical questions of nineteenth-century Russian literature: who (or what) is guilty? Of primary blame, according to the author, is the ontological version of Marxist philosophy that was put on record by Stalin in "Dialectical and Historical Materialism" (1938) in opposition to the critical versions which Lecourt associates with Marx, Engels, Lenin, and Mao. The ontological version of Marxism implies that laws of matter are stated by the dialectic and, accordingly, make the Soviet version of Marxist philosophy a supreme authority in the field of the natural sciences. What is more, this ontological approach was successfully coupled with a presentation of Lysenko's theory as a new kind of science, a so-called proletarian science. Finally, the author traces the origin of the idea of proletarian science to Alexander Bogdanov's empiriomonism.

In his preface to *Proletarian Science?* Louis Althusser ridicules the Soviet tendency to make the "cult of personality" responsible for all the shortcomings of Soviet society. Unfortunately, in the final analysis, Lecourt's book almost reduces itself to the same dogmatic assertion, making a sacred cow of Lenin and proceeding only very slowly to a drastic re-evaluation of the relationship between Marxism and science. One cannot help wonder why so dynamic and revolutionary a doctrine as Marxism-Leninism becomes conservative and dogmatic whenever it confronts fundamental and independent intellectual activity such as art or science.

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DENNIS J. DUNN, editor. *Religion and Modernization in the Soviet Union*. Boulder, Colo.: Westview Press. 1977. Pp. x, 414. \$21.75.

In March 1976 the American Association for the Advancement of Slavic Studies sponsored a conference at Southwestern Texas State University to investigate the neglected subject of the relation between modernization and religion in the Soviet Union. This book contains eleven papers which were presented at that meeting.

The editors of the volume, Dennis J. Dunn and Donald W. Treadgold, collaborate on an introduction to the subject. Dunn argues that generally speaking Western historians have characterized modernization as involving a decline in religion's influence in society. In nineteenth-century Western Europe the people who moved into the cities to work in the factories certainly became more secular in their concerns. This same phe-

nomenon, however, has not been repeated in the Soviet Union, despite rapid industrialization and the overt hostility of the government. Just beneath the surface of Communist society, Dunn and the contributors to this book see Holy Russia still alive and breathing.

The attempt to analyze the special situation of Soviet religion's response to modernization was an easier task for some authors than for others. Although all of the articles are of high quality, several appear to touch the theme of the work only tangentially.

Among the best is William C. Fletcher's essay on the "demodernization" of the Russian Orthodox Church. He takes the point of view, contrary to the opinion of Alexander Solzhenitsyn, that the submissive attitude of Church leaders to the state, adopted since the time of Metropolitan Sergei, has been a blessed necessity for the continued existence of Orthodoxy. He believes that the popularity now enjoyed by the Church among intellectuals and young adults can, in large measure, be traced directly to the inability of the Church to modernize due to government restrictions. Orthodoxy's traditionalism, its conservatism, even its lack of relevance to modern man have made it attractive to the two groups that presumably should have been most affected by secularization.

The same theme is repeated in the articles on Soviet Muslims, Jews, Catholics, and Protestants. All of these communities have been physically isolated and intellectually frustrated. While their more vigorous leaders have been extinguished, those who speak in platitudes have been rewarded. Thousands of places of worship have been closed and for decades believers have been bombarded with antireligious propaganda. But no one in the Kremlin is smiling, because the results of all these measures have often been counterproductive. In relegating religion specifically to worship, the Communists have made the mistake of forcing priests, rabbis, and mullahs to do what they do best. After reviewing the materials in this book, it would appear that the helplessness of the Soviet believer should not, in any way, be equated with hopelessness.

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DOROTHY ATKINSON *et al.*, editors. *Women in Russia*. Stanford: Stanford University Press. 1977. Pp. xiii, 410. \$18.75.

In this country, interest in the situation of women in Russia ebbs and flows, but mostly it trickles. Popular works on the subject appear periodically

(the latest is William Mandel's *Soviet Women*); scholarly interest has been minimal. Recently, the publication of such works as *Five Sisters*, *The Women's Liberation Movement in Russia*, and *Women in Soviet Society* suggests that this topic will begin to get the attention it deserves.

Women in Russia is a worthwhile addition to the literature on this topic. The outgrowth of a 1975 conference at Stanford, the book includes several articles on prerevolutionary Russia, but is mostly concerned with the Soviet period. Russian women are the focus; there is little information about the non-Russian peoples who currently comprise almost half the population of the USSR. In general, this work is reminiscent of other collections of this type. Its title promises far more than it can deliver; it is a beginning, a guide to some of the many fruitful areas of exploration in a field still open to pioneers.

The section on prerevolutionary Russia contains four articles. Three provide information and insights heretofore not available in English. Beginning with the Amazons (thought to have lived in southern Russia), Dorothy Atkinson sketches a useful portrait of "Society and the Sexes in the Russian Past," skillfully weaving together material from folklore and law. Atkinson's presentation is marred by its focus on upper-class life; too little attention is paid to the far more numerous peasant women. Richard Stites's discussion of "Women and the Russian Intelligentsia" ranges across the entire spectrum of responses to the woman question, from conservative antifeminism to socialism, demonstrating an impressive knowledge of the sources. Rose Glickman ably analyzes the methodological problems in studying "The Russian Factory Woman, 1880-1914."

The articles on the Soviet period are largely concerned with socialist practice rather than theory. The contributors acknowledge what has been done. In comparison to the U.S., the Soviet achievement is impressive. The major feminist battles here—ERA, affirmative action—are ancient history in the USSR. There has been great progress in integrating women into the paid labor force and into such traditional male bastions as engineering, science, and medicine. Divorce and abortion laws are much more liberal; child care is cheaper and more accessible. Still, much remains to be done. Many of the articles mention the "double burden" of emancipation—paid work outside the home with no reduction in unpaid "women's work" in the home. The double burden and the government's unwillingness to extend the concept of emancipation to include the re-examination of sex roles are cited by many of the contributors as keys to the substantial inequities still remaining for Soviet women.

The material presented in the post-1917 section is rich and varied. There is information about development strategy, rural women, women in the party, women in the professions, inequities in pay, educational policies, children's readers, social services for women, sex in Soviet law, and the early marriage-law debates. Though there is some overlap, the articles generally complement each other well.

Several caveats are in order. Little attention is paid to ideology; the sole article on Marxist theory, while excellent, discusses only Marx, Engels, and their German disciples. The Soviet commitment to women's equality too often is viewed as simply a response to immediate development needs. The notion that the double burden is a major barrier to the career advancement of all women is uncritically accepted. Yet statistics cited by some contributors indicate that a large number of Soviet women are unmarried, divorced, or widowed. We need more precise information about the double burden and its effect on different groups of women. Similarly, several articles assessing the current situation of Soviet women imply that the mood is more feminine than feminist. That Soviet women do not call themselves feminists is clear; that they can be quite vocal in defending their economic independence against various compulsory motherhood schemes is also clear. A more sophisticated analysis of these issues would have been helpful. Finally, the price of this book equals a day's pay for many women workers. This makes it virtually inaccessible to those who might be most interested in learning about alternatives to our system.

With all this, *Women in Russia* is still an important work, and should be required reading in courses about Russia and the USSR. Hopefully, this book will help remedy the general neglect of this subject in most such courses.

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NEAR EAST

A. S. SILIN. *Ekspansia germanskogo imperiializma na Blizhnem Vostoke nakanune pervoi mirovoi voiny (1908-1914)* [The Expansion of German Imperialism in the Near East on the Eve of the First World War (1908-14)]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 357. 2 r. 62 k.

In this detailed work A. S. Silin, author of *German Expansion in the Middle East at the End of the Nineteenth Century* (1971) and of numerous articles on the subject of German-Ottoman relations, continues his examination of the "enslavement of Sultanic Tur-

key by German imperialism." His latest book deals with events that preceded World War I: the von der Goltz mission, German naval policy in regard to the Ottoman Empire, "The Struggle of German Armament Monopolies for the Turkish Market," the Baghdad railway, the von Sanders mission, and Germany's role in bringing Turkey into the war.

The von der Goltz mission was perceived by Russian military and diplomatic officials as a German triumph in the struggle with Britain and France for domination of the Ottoman Empire. Silin, however, stresses the anti-Russian character of German military assistance to the Turks to whom the Germans assigned the role of "riveting to the Caucasian front large Russian forces and thereby helping German and Austrian troops in their operations on other fronts" (p. 36).

The author examines negotiations, agreements, and contracts among various parties, including the British, who appear to have played, next to the Germans, a predominant role in Ottoman affairs. The economic interests of the great powers are not ignored. Silin shows that both German and British war industries reaped large profits from supplying Turkish armed forces. He sees the Baghdad railway as a means by which German capital achieved economic domination at the cost of the impoverishment of the Turkish masses. Though this point is argued at some length, it is poorly documented, the issue being settled by quoting Marx, Engels, and Rosa Luxemburg (pp. 176-78).

In spite of all the references to the "works of the founders of Marxism-Leninism," the book was written from a Russian nationalist point of view. Russia's imperialist aims are minimized, those of Germany and Britain exaggerated. Silin repeatedly emphasizes the large size of the von der Goltz mission, creating the impression that it included hundreds of German advisors, instead of the rather modest group of twenty-five to thirty men. He flings pejorative epithets at the imperialists and feudalists, provided they are not Russian. The poverty of Turkish statesmanship, and the low character of Turkish leaders, are explained thus, "Only in the conditions of backward sociopolitical relations predominant in Turkey could people of such type [Enver, Talaat, Cemal] take power and command the destinies of the community unchecked" (p. 237). Such a primitive linking of "sociopolitical relations" to the human quality of national leaders would raise serious problems for the author should he apply his method to Russia in 1929-53 or to Germany in 1933-45.

The book contains a number of inaccuracies (for example, "in the Muslim calendar the year starts on March 1" [p. 151]); transliterations are inconsistent and do not seem to follow any recognizable

system; documentation is often sloppy—a newspaper account or an opinion of some anonymous agent presented as serious evidence; the language is loaded with terms designed to provoke loathing for all those of whom the author does not approve. The moral of the work is given in its concluding sentence: “One may suppose that the bloody experience of military collaboration with foreign imperialists that brought the Turkish people so many misfortunes and so much suffering has not been forgotten in today’s Turkey.”

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KAMAL S. SALIBI. *Crossroads to Civil War: Lebanon, 1958–1976*. Delmar, N.Y.: Caravan Books. 1976. Pp. viii, 178. \$15.00.

This is perhaps the first book to appear in English on the recent civil war in Lebanon. Kamal S. Salibi finished writing his account in late January 1976, long before the civil war was brought to an end in November 1976. The major part of the book consists of a useful background on the politics of Lebanon and tension between its religious communities and parties from the end of the confrontation in 1958 to the beginning of the civil war on April 13, 1975. It also includes details about the growth of the Palestinian commando movement in Lebanon and the different attitudes of the various communities and parties toward the Palestinian military presence and its activities.

The smaller part of the book deals with the civil war itself in its various stages and on its various fronts. The dramatic chronicle of events, accompanied by helpful maps and illustrations, includes a description of the role played by the Christian militias, the Muslim and radical militias, and the Palestinian forces in the ferocious fighting, as well as the role of Syria in acting as a mediator and in allowing Palestinian armed forces to enter Lebanon. The author contributes helpful insights on the helplessness of state authorities and on the Maronite Christian view of the absolute sovereignty of Lebanon as a state in contrast with the Muslim view influenced by their “unbounded zeal for Arabism.” He explains how the weakened Muslim leaders lost their independence and became a political instrument serving the commando cause within the Lebanese system and how Palestinian issues became confused with Lebanese domestic issues. He shows that the Maronite Katā’ib and its leader Pierre Jumayyil were receptive to moderate demands for reform but preferred to destroy the country themselves or force its partition rather than yield to radical demands and to Palestinian encroachments.

In his conclusion, Salibi blames not only the Lebanese for showing “complete incapacity to manage their country properly” but also the Arab regimes for not respecting the neutrality of Lebanon in inter-Arab conflicts. They tried to get it militarily entangled in the Palestine question, and they took liberties with the delicate mechanism that kept the country going until they caused its destruction. The author would have been more helpful if he had described in some detail that delicate mechanism with its delicate confessional balance, its government by compromise, and its relative success, in spite of its defects, until massive external intervention occurred in 1958 and repeatedly after 1969. The book will be more valuable when revised to include a complete account of the civil war and its aftermath.

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NADAV SAFRAN. *Israel: The Embattled Ally*. Cambridge: Belknap Press of Harvard University Press. 1978. Pp. ix, 633. \$18.50.

The year 1978 marks the thirtieth anniversary of the founding of Israel, and there has been an outpouring of books to celebrate the event. Nadav Safran’s large book is one of these. The author has written something less than a history of Israel and something more than a narrative of American-Israeli diplomatic relations. In addition to discussing Israel’s international problems, he treats the philosophical foundations of Zionism, the Jewish relationship with Palestine, and the founding of Israel and provides broad-brush sketches of the geographic, demographic, economic, social, and political problems of the country.

In addressing Israel’s internal history, Safran focuses on certain contemporary issues. An Oriental Jew who resided in Israel for a short period in the 1940s, the author laments the “psychic gap” between Oriental and European Jews. He admits, too, that Israel’s Zionist character gives it “a certain exclusivist tendency that barred the Arabs from full membership” (p. 103). Safran delineates the demographic problem posed by possible annexation of the West Bank and Gaza Strip—territories that would add approximately one million Arabs to an Arab minority that already approaches the half-million mark. By 1990 the Arabs, by virtue of a high birth rate, would comprise nearly 50 percent of the population of Israel. Although enjoying a diversified, rapidly growing economy, Israel’s high rate of investment is based in large measure on private and public foreign sources. In order to escape continuous economic

dependence on the United States, the author asserts that Israel must "undertake drastic reforms." His discussion of Israel's parliamentary democracy, with its ideologically oriented, multiparty system, is both detailed and stimulating. He describes the intense internal pressures on the people brought on by a high rate of immigration, the conflict over the role of religion in society, the burden of self-defense, and the effects of an inflated economy.

In addressing Israel's international problems, Safran provides his reader with an analysis of its relations with the nations of the Middle East, and he traces the evolution of U.S.-Israeli relations. He asserts in his preface, "About the United States and Israel one might say that seldom in the history of international relations has such a world power been involved so intensely for so long with such a small power" (p. vii). Safran posits that in the ten years since the 1967 June War there has been an identity of interests between the United States and Israel. He asserts that "the Yom Kippur War provided an occasion for the most vivid expression of the American-Israeli alliance that had been developing since the Six Day War" (p. 387). With respect to the present state of negotiations in the Middle East, Safran concludes that "the United States and Israel must strive to advance a settlement in every possible way; United States policy must have a dual orientation, on Israel as well as on Arab states; and the United States must be prepared to inject major 'inputs' to make possible Arab-Israeli agreement" (p. 395).

In using the term "American-Israeli alliance," it seems that Safran is overstating his case. Even the use of "tacit alliance" to describe the relationship is somewhat misleading. At no time does Safran spell out the terms of the alliance, and he does not tell us when this alliance came into force. Although he presents some provocative ideas on the tortuous development of the Middle Eastern power configuration during the past decade, there are times when his conclusions might not bear up under close scrutiny. William B. Quandt's recent work on the American role in the Middle East presents a more balanced treatment of the decade in question, and his use of the term "special relationship" to characterize the American-Israeli connection seems more applicable than does the term "alliance."

The general reader will find this book interesting, but at times its length and detail detract from the flow of the narrative. The specialist in the field will find the book provocative and its bibliography adequate but will lament its lack of documentation.

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AFRICA

ROBIN LAW. *The Oyo Empire, c. 1600-c. 1836: A West African Imperialism in the Era of the Atlantic Slave Trade*. (Oxford Studies in African Affairs.) Oxford: Clarendon Press. 1977. Pp. xiv, 340. \$24.95.

In late precolonial West Africa the region known as Lower Guinea was dominated by four territorial empires: Asante, Dahomey, Benin, and Oyo. Of these the first three have been much studied, and comprehensive histories of all of them have appeared during the last ten years. But the same is not true of Oyo. For detailed information about the most powerful state that ever arose in what is now Western Nigeria it has heretofore been necessary to consult Samuel Johnson's *History of the Yorubas*, which first appeared in 1921 and was written nearly a century ago.

Historians of West Africa have neglected Oyo for many reasons. Its capital, Oyo Ile, lay a long way from the coast and, until the last years of the empire, no foreign visitors traveled there or, if they did, left no record of their experiences. There is virtually no contemporary written evidence, except for second-hand and hearsay comment, about Oyo in its heyday. The oral traditions relating to the period of the empire are now more than a century and a half old and are much corrupted by suppressions and interpolations. Archeology has revealed a little of the region's material culture but almost nothing of its history. Most important, since Oyo ceased to exist half a century before the age of imperialism began, there is no record of diplomatic or military contact between Oyo and the incoming Europeans. The kind of evidence that has been crucial for the successful reconstruction of the history of Asante, Dahomey, and Benin simply does not exist.

The task facing the author of *The Oyo Empire* was, therefore, unusually difficult, and it should be said at once that he has been equal to it. Meticulously researched and lavishly annotated—there are nearly seventeen hundred footnotes—the work reveals an impressive mastery of Yoruba culture and history. It deals successively with the origin, constitutional arrangements, and style of government of the Oyo kingdom; with the processes of conquest and incorporation by which the kingdom became an empire; with the empire's political, military, and economic history in the seventeenth and eighteenth centuries; and with its decline and collapse in the 1820s and 1830s. At many points Robin Law corrects erroneous but long-held views. He shows that the Alafins (kings) of Oyo had no true claim to "the heritage of Oduduwa," that is, to be the senior rulers among the kings of the Yoruba; Oyo owed its position to military

power, not mystic right. He shows that the extent and might of the empire have been much exaggerated by previous writers because of an uncritical acceptance of the claims of Oyo's court historians. And he shows that the chronic instability of the empire in the eighteenth century (about which there has been much speculation) resulted primarily from the resentment felt by leading nonroyal chiefs against the Alafins' use of slave officials to increase the royal power.

The Oyo Empire is a work for specialists. Readers should have a solid grounding in the political geography of precolonial Yorubaland if they are not to be overwhelmed, particularly in the middle chapters, by the denseness of the material. But the book is a fine achievement. It fills a gap in the historical literature on West Africa that has existed too long.

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EUNICE A. CHARLES. *Precolonial Senegal: The Jolof Kingdom, 1800-1890*. (African Research Studies, number 12.) Boston: African Studies Center, Boston University. 1977. Pp. xi, 163. \$7.50.

The kingdom of Jolof occupied a dry, sandy area in the interior of northern Africa. It was briefly the center of a medieval empire, but its resources were limited by low rainfall. And, with the beginning of the slave trade, its more populous coastal neighbors threw off Jolof hegemony. By the nineteenth century, it was weak, rent by internecine conflict, dominated by more powerful neighbors, and torn by profound religious divisions. In the latter part of the century, however, these religious conflicts were resolved by an able and forceful Muslim ruler, Albuuri Njay, and Jolof briefly became one of the major barriers to French conquest. Its barrenness and isolation, which had been factors in its weakness, suddenly became sources of strength along with Albuuri's shrewd use of its surplus of horses. Ironically, Albuuri never actually fought the French until it was too late, and then when French columns approached his capital he fled to the east, where he joined Ahmadu of Segou in a last-ditch struggle that made him a romantic hero for modern nationalists.

Eunice Charles tells this story well in her monograph, an abridged version of a Boston University thesis. It chronicles the divisions and internal conflicts, describes the triumph of Islam, and paints a vivid picture of Albuuri. It is based on research in both the oral tradition and the fairly substantial archival materials for the nineteenth century.

If this study disappoints, it is largely because the reader is never sure why a young American scholar would choose to study a small and distant corner

of the world. She does not confront the larger questions involved in Jolof history. She is good on detail, weak on the larger pattern. Thus, she describes Jolof's trade links with France and with its neighbors, but never moves to analysis of its political economy. Events are never analyzed in terms of control of political and economic resources. She never comes to grips with the reasons for Jolof's instability, and she sees its resurgence largely in terms of Albuuri's undeniable abilities. Though somewhat better on islamization, she probably exaggerates the role of the *tijanniya*.

There is nevertheless some value in shining a small light into a hitherto dark corner. Charles has collected and presented the data upon which she and other scholars may want to reflect more profoundly.

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POLLY HILL. *Population, Prosperity, and Poverty: Rural Kano, 1900 and 1970*. New York: Cambridge University Press. 1977. Pp. xviii, 240. \$17.95.

In *Population, Prosperity, and Poverty* Polly Hill continues her dismemberment of "the myth of the amorphous peasantry" and probes further into the structure of rural Hausa society. Along the way she drives a few more nails into the coffin of the two-sector economic model in which "urban" is defined as industrial, market-oriented, and modern and "rural" as agricultural, subsistence-oriented, and traditional. This study of Doraye in the Kano Close Settled Zone (KCSZ) and her earlier study of Batagarawa in rural Katsina (*Rural Hausa: A Village and a Setting* [1972]) confront various pronouncements of received doctrine about Hausa society and flesh out a growing knowledge of precolonial Nigeria.

Hill argues that the "present-day sociological stability of stagnating, impoverished, long-established, overcrowded rural communities" in the KCSZ derives basically from economic relationships that had evolved before 1900 (p. 95). Most long-distance trade in Kano Emirate in 1900 was rural-based. Cotton cloth, cotton garments, and leather goods were manufactured in the countryside, not in the city, and Hausa long-distance traders also found it more convenient to live in the country. Some present-day markets in the Zone served caravan routes that did not go into the city at all. The Zone became a center of immigration in its own right because the high population density provided exceptional opportunities for craftwork and trade. This immigration was reinforced by the introduction of thousands of farm slaves.

In the twentieth century, slavery gradually dis-

appeared, and the large slave-operated farms were broken up. Economic opportunities declined, but population continued to grow. Today, Doraye farmers no longer own cattle and must rely on household sweepings and night soil carried by donkeys from Kano to fertilize their fields. The general level of income and the size of land-holdings have declined and are now much lower than in Batagarawa. Labor is under-utilized throughout the Zone.

Despite the great decline in prosperity, emigration is infrequent and usually surreptitious. Fathers cling more tightly to their sons as their property diminishes, and the departure of a son is a cause for shame among those who stay. No one migrates to farm in less crowded areas elsewhere because he cannot accumulate funds to support his family while establishing a new farm and he has no kinsmen to go to.

Another apparent consequence of poverty is the existence of big houses that may contain ten or more married families. Most of the houses date from sixty to one hundred years earlier, when each could be surrounded by ample farmland; they survive because land shortage has caused security in family to become a substitute for security in property.

The richness of information in *Population, Prosperity, and Poverty* is barely suggested by a brief review. Other major topics are Lugard's misinterpretation of Hausa Fulani government (presented in a chapter entitled "Indirect rule as rural non-rule"), the nature of Hausa farm-slavery and the position of the descendants of slaves (no different from the condition of descendants of freemen), land-tenure rules, and the extent and economic significance of inequalities in income and wealth.

Hill writes concisely and every sentence must be read attentively to get full value. Nor does she always point out the implications of the evidence in as much detail as the reader might like. As a consequence, her valuable research findings have not had as wide an impact as they should have. Nevertheless, the mounting body of evidence she has presented in this and earlier volumes about the true nature of West African rural economies is slowly penetrating the understanding of African policy makers and cannot help but benefit the West African people.

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ARTHUR J. KNOLL. *Togo Under Imperial Germany, 1884-1914: A Case Study in Colonial Rule*. (Hoover Institution Publication, number 190.) Stanford: Hoover Institution Press. Pp. xi, 224. \$8.95.

In his introductory chapter Arthur J. Knoll quotes L. H. Gann's remark that a great deal of ink has been spilled on the German colonial empire. It is true that few aspects or periods of African history have been as densely covered in monographic literature as the thirty-year colonial rule of Wilhelminian Germany. Thus it is not surprising (nor grounds for criticism) that this study offers no new insights into the basic nature, motives, and structure of the German colonial enterprise. Its essentially economic motivation, pragmatism, modesty of scale and means, and low priority among the political concerns of the government have been discussed in numerous studies, some of which the author usefully reviews in his first chapter.

Nevertheless this work, originally a dissertation, is useful as a contribution to the history of Togo. Although Knoll cautions that his sources—mainly the colonial archives now split between Lome and Accra, and publications of the German government and various colonial groups—have more to say about the colonizers than the colonized, his book is of interest to the student of African history, as well as to readers specializing in colonial policy and institutions. The focus is on German aims, actions, institutions, and personalities. (The energetic, scholarly, and liberal-minded Julius Count Zech receives particular, deserved attention.) But the effects of German endeavors—administrative, economic, and missionary—on various indigenous groups, and their responses, are by no means neglected.

Perhaps somewhat overconcerned with the rather old-fashioned question of the quality of German rule in comparison with other contemporary colonial regimes, the author reaches rather positive conclusions for this German "model colony": government was honest and efficient, effectively concerned with developing the resources of the territory, and possessed of some paternalistic instincts toward its people. Large-scale violence, so much a feature of German rule in other parts of Africa, was absent, in part no doubt because of the absence of European settlers. The military establishment was minimal. On the negative side, amply documented by Knoll, was a pattern of unabashed domination by metropolitan economic interests, represented in Togo largely by the Hanseatic trading firms, and of unquestioned social and legal inequality, harsher and more open, at any rate, than in neighboring British colonies.

On the whole, the book's organization and style are unexceptionable, though one irritatedly notes the frequent use of "elite" as a singular noun, which remains a barbarism for all its frequent use in works by political scientists.

WOLFE W. SCHMOKE
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ALISTAIR HORNE. *A Savage War of Peace: Algeria, 1954-1962*. New York: Viking Press. 1978. Pp. 604. \$19.95.

For France the war in Algeria was a watershed, marking the end of over a hundred and twenty years of French control of the Maghrib, causing the death of one Republic, and covering both France and Algeria in a cloth of mourning. One can still feel the presence of the war in France, its scars unhealed after a decade and a half. For those of us who lived through all or part of the conflict, it is clear that it became, as General de Gaulle later said, a sterilizing obsession. In his new, massive work, *A Savage War of Peace: Algeria, 1954-1962*, Alistair Horne has tried to chronicle and to explain those eight years of war. At the outset, anyone involved with the history of Algeria and North Africa will have to admit that to record in detail and in some readable form the history of the Algerian war is a monumental task, a task filled with many pitfalls. Yves Courrière, in his excellent four-volume work *La Guerre d'Algérie* (1968-1971), had the courage to tackle the project, and the result was a boon for students of the period. In his preface, Horne relates that he worked closely with Courrière, and the relationship is apparent in the book. Two themes run through *A Savage War of Peace*: first, the war in Algeria was the last of the great colonial wars, surpassing in historical meaning the French involvement in Indochina and serving as a transition between the colonial conflict and the new urban terrorist conflict of today; and, second, the war was a tragedy for France, Algerian Muslims, Algerian *pieds-noires*, and the world in general. No reader could quarrel with Horne's ability to amass the facts of the conflict or with his method of conveying the tragedy of the conflict.

There is, however, in the well-related mass of facts a certain lack of understanding of the revolution, the *colon* resistance, the OAS, and the final acts of self-destructive violence. To give meaning to the war, or perhaps wars, in Algeria it would be necessary to look back at the repressive regime in Algeria in detail. That a bloody revolution in Algeria was inevitable at some point is clear. The harsh and quite discriminatory *Code de l'indigénat* of 1881 began the process of politically separating the Muslims, and in 1884 the colonizers, through law, reduced Algerian Muslim participation in municipal councils from one-third to one-fourth the number of members. The suppression of the colonized had a long history, and the fermentation that led to the violence of 1945 and the outbreak of the revolt in 1954 was a long time coming.

Political repression was married to a system that robbed the Algerian "native" of his humanity. From Yvonne Turin's *Affrontements culturels dans*

l'Algérie coloniale: Écoles, médecines, religion, 1830-1880 (1971) and from the articles on cultural disrespect by Alf A. Heggoy and others, one can see the patterns forming that brought about the final crisis in 1954. The historian cannot forget the die-hard representatives of the *colons* who sat in the Chamber of Deputies and represented only *pieds-noires* interests for so many years, reinforcing the rule of the minority, denying the true spirit of meaningful reforms, and degrading the ideals of the French Revolution.

Horne's *A Savage War of Peace* is a well-written work that deals mainly with the events from 1954 to 1962. There is, however, a lack of that vitally important analysis of the long history of events in Algeria that caused the deep-seated feelings that finally brought about the insurrecting having such far-reaching consequences for France and Algeria. There is certainly enough horror related in the book to convince any reader of the intensity of the war in Algeria, but one is left with a real feeling that the historical background is vague. On the other hand, Horne has undertaken a monumental task—one which he set for himself—to write a single-volume, complete history of the war. He did accomplish that objective by presenting the basics of the war in one well-written tome. Perhaps it is impossible to write satisfactorily a single history of the war when so many who lived it and fought in it are alive and when so many documents still remain closed. But Horne has certainly made that first step toward a comprehensive history of the conflict itself.

JAMES J. COOKE
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JOHN J. STREMLAU. *The International Politics of the Nigerian Civil War, 1967-1970*. Princeton: Princeton University Press. 1977. Pp. xix, 425. Cloth \$26.50, paper \$9.95.

The Nigerian civil war, which pitted the Federal Government against Biafra, was a long and bloody variant of a generalized conflict between the demands of new African states for integration and stability and the demands of communal groups for national self-determination. A basic lesson of the war is that African secessionist movements have almost no hope of succeeding without international recognition and support. Biafra needed not only to break away from Nigeria but also to break into the international arena. Although it did not succeed in either of these goals, Biafra did hold out—with the aid of nongovernmental international charity—for more than two and a half years, from May 30, 1967 until January 13, 1970.

John J. Stremiau's work is carefully researched

and documented; he interviewed over two hundred participants (including Gowon and Ojukwu) and had access to confidential material from both sides. Stremlau tells the story of the secession and its international ramifications in chronological order, beginning with regional conflict and coups and ending with capitulation. He shows how both parties attempted to elicit international aid, especially from the OAU and other African countries. He brings out how Britain and the Soviet Union joined in supporting the Nigerian side and how a few African countries, motivated as much by humanitarian impulses as by reasons of state, supported Biafra by according it international recognition. Stremlau argues convincingly that the reorganization of Nigeria into twelve states, giving minorities a modicum of autonomy, undermined Biafra's claim that it represented the minorities of the former Eastern Region but also strengthened its resolve to resist Nigerian demands for fear of losing its industrial base, especially at Port-Harcourt. He demonstrates how lack of military success after the defeat at Ore, a turning point in the war, vitiated Biafran pleas for recognition and how the lack of recognition, in turn, stemmed the military aid Biafra needed to carry on the war.

Stremlau is most interesting and controversial when he discusses humanitarian aid to the beleaguered secessionists. He points out that over two hundred and fifty million dollars was channeled to Biafra from sources such as Caritas and the World Council of Churches and repeats the Federal argument that, by prolonging the war, such aid prolonged the suffering. Yet it should be recalled that a major issue of this war was the reaction of the international community to the threat of genocide, and here Stremlau's work is weakest. He seems to argue that, since Biafra successfully used the fear of genocide to mobilize the masses and to stimulate external aid, the fear was groundless. To support his claim he points to the fact that genocide did not take place after the Nigerian victory, and he argues that it was never intended because General Gowon had humanitarian impulses. The fact that genocide did not take place does not prove it was never a possibility nor that it was not genuinely feared. After the events of summer and fall 1967, Biafran propaganda no doubt had an easy time of it. As to General Gowon's intentions, who was to say in 1968 that he could prevent his troops from attacking Ibo civilians as they had earlier attacked Ibo officers?

The author's careful research allows him to tell this story at a level of detail not previously achieved; his brief but perceptive sketches of Gowon, Ojukwu, Awolowo, and Kaunda lend rich texture to his work. It is doubly disappointing, therefore, that this work fails to address explicitly

some of the major theoretical issues raised by the Biafran secession or to show understanding of the fear of genocide that initiated and sustained the secessionist movement.

ROBERT MELSON
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ASIA AND THE EAST

ROXANE WITKE. *Comrade Chiang Ch'ing*. Boston: Little, Brown and Company. 1977. Pp. xxvi, 549. \$15.00.

Carlyle once wrote that "history is the essence of innumerable biographies." If that is true, then *Comrade Chiang Ch'ing* should add breadth to our understanding of the Chinese Revolution and the contemporary political scene in China. For it to do so, though, we must know where to place Chiang Ch'ing in the context of twentieth-century Chinese history. Was she a cultural-political leader, a revolutionary, a feminist, or merely the power-hungry wife of China's greatest leader? Roxane Witke wants to place her in all these categories, yet the evidence she presents does not support her contention.

Witke went to China in 1972, as a guest of the People's Republic, to study younger women involved in the revolution and in the Central Committee of the Chinese Communist Party (CCP). Once there, Chiang Ch'ing seized on her presence and spent a week in marathon sessions with Witke, in order to make her "bid for historical recognition" (p. 4). When Witke's significant book was published in 1977, it was no longer a study of women in China but rather a history of the struggle of one woman during the long revolution. Although the experiences of political women in China are still, unfortunately, largely unknown, Witke has given us a history that she hopes "carries clues to understanding the fundamental dilemmas of women and power in revolutionary China" (p. 34). The book does indeed aid us in discovering more about the wealth of conflict over power in China, but it tells us little about the roles and struggles of women.

Publication of this biography of Chiang Ch'ing was obstructed: translated transcripts of all but the first session, including those taking place in Chiang Ch'ing's villa, were not sent on to Witke; both Chinese and American officials encouraged her not to publish; her Harvard office was raided. The well-written volume, replete with unique photographs, glossaries, and calligraphy is testimony to the will and perseverance of the author.

Comrade Chiang Ch'ing should be read primarily as a self-serving political document. Chiang

Ch'ing told her story, deleting, ignoring, and expanding where she saw fit. We follow her from her early years in Shantung, through her attempts to join the CCP and carve out an acting career in Shanghai, to Yen'an where she met Mao and helped create socialist culture, and to Peking and back to Shanghai where, in the 1960s, she made her move for political and cultural leadership. Chiang Ch'ing expresses in detail her struggles and conflicts with various party and cultural leaders, and exults in her victories and their downfall in the Cultural Revolution. In images and actions, she emerges as a vengeful, courageous, heterodox cultural leader. What she fails to express, although Witke does her best to present her in this guise, is any notion of feminism or even of how her own role—as cultural leader, one of the very few female members of the Politburo, and leader of the Cultural Revolution—influenced the status of women in Chinese society. Her story is vital and compelling, one-sided and reconstructed without doubt, but of interest as a chronicle of a power struggle and an ideological conflict over values and culture.

It is unclear how history will judge Chiang Ch'ing. She has been defamed and denounced as the evil leader of the "Gang of Four" in China; from our perspective, her struggle to revolutionize the Chinese superstructure of art and culture and her belief that the superstructure can change the economic base of society appear the most enduring of her legacy. Witke has done an excellent piece of work elucidating Chiang Ch'ing's experiences and explaining the historical interrelationship of politics and art in revolutionary China. But Witke often veers from this perspective to a view of Chiang Ch'ing as a feminist revolutionary, and thus confuses the significance of Chiang Ch'ing's leadership. Witke's apparent attempt to integrate her original topic on female revolutionaries in China with the experience and politics of Chiang Ch'ing does no justice to the former and misconstrues the latter.

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JÜRGEN DOMES. *China after the Cultural Revolution: Politics between Two Party Congresses*. With a contribution by MARIE-LUISE NÄTH. Translated by ANNETTE BERG and DAVID GOODMAN. Berkeley and Los Angeles: University of California Press. 1977. Pp. x, 283. \$15.00.

Writing recent history is necessarily perilous, all the more so when an author attempts a scientific analysis purporting to give a correct view of a short

period and to indicate future trends. Jürgen Domes's *China After the Cultural Revolution* covers the turbulent years between the People's Republic's Ninth Party Congress in the spring of 1969 and the Tenth Congress in the fall of 1973. Originally published in Munich in German in 1975, this English-language edition includes an introduction and an epilogue written after Mao's death. A separate section, contributed by Marie-Luise Nāth, covers foreign policy since 1958, the year the authors consider signal for understanding Lin Piao's final rise and fall. Rarely is drastic change anticipated. Dated October 1, 1976, Domes's epilogue did not foresee the arrest five days later of Mao's wife, Chiang Ch'ing, and of the three other leaders who made up the so-called Gang of Four or the immediate accession of Hua Kuo-feng to the party chairmanship.

But the very fact that the book came out before Hua's regime began to rewrite history with a Stalinesque ferocity enhances its historical interest. For Domes, a leading West German scholar of contemporary Chinese affairs, has based his book upon the Chinese media and Western scholarship that prevailed in Mao's times. Hua is mentioned only in passing as the first secretary of Hunan's Provincial Party Committee as of December 1970, and as the first vice-chairman of the Central Committee, member of the Standing Committee of the Politburo, and premier of the State Council as of April 1976. Despite these formidable promotions, there is no hint that Chairman Mao also had secretly appointed him as his successor. But right after the American edition went to press, Hua's designated succession to the party chairmanship became a major datum of world history.

Few authors have captured the high drama and historical resonance of the four years between these party congresses. Nor has Domes in this densely written book—a tating together of hundreds of names of persons, organizations, and meetings—explored the individual values and complex relations of the leaders in question. Standing back from the welter of details, we realize that during these years the frenzy of the Cultural Revolution dissipated. For the masses most formal education, theater, and entertainment were suspended, and general reading was prohibited. Marshal Lin Piao continued to mount his fantastic personality cult of Mao, who had named him successor. Through devious and still mysterious means Lin had sought to hasten the chairman's demise. But he failed and reportedly crashed trying to escape by air over Mongolia. Domes rightly questions the veracity of official explanations for his bizarre disappearance, particularly the allegation that he was in league with the Soviet Union. More than other observers of high Chinese poli-

tics, Domes notes how Mao's active participation in government fell off after the mid-1960s. Before and during the Ninth Party Congress much noise was made about rejuvenating China's leadership. But, as Domes explains through tables, the words were largely fatuous. Despite the radical egalitarianism of the rhetoric of the Cultural Revolution, in actuality few youths, women, and poor peasants subsequently rose to power. Token appointments were made, but the average age of the leading cohort continued to rise, resulting in the present gerontocracy.

Domes's most provocative arguments are about Lin Piao's political passions. Wisely, Domes rejects the simple "two line" theory by which Mao explained all political struggles of a half century. Nor does he find satisfactory the radical versus moderate view that was popularized by American academics and journalists. Mao was, as Domes observes, complex, allowing his followers to play out his various parts. Although Domes repudiates these dualistic conceptualizations, his own analysis is similar and hardly clearer. On the one side there were the leftists headed by Mao, espousing the "mobilizational development concept" of the Three Red Flags Program (better known as the Great Leap Forward) of 1958. Its commitment was reasserted during the Cultural Revolution of the late 1960s, at Lin Piao's zenith in 1969-70, and briefly again in the spring of 1973. Lin Piao, Ch'en Po-ta, Chiang Ch'ing, and the other Cultural Revolution leaders were its principal advocates. What Domes finds so notable about Lin was his determination to revive the 1958 model—especially its labor-intensive agriculture and military dominance—after more than ten years had passed and his comrades' priorities had changed. On the other side were the promoters of "readjustment," of the "New Course line," as Domes oddly calls the "pragmatic" types. They were knocked down during the onslaught against Liu Shao-ch'i in the mid-1960s, though Chou En-lai managed their partial resurgence in 1970-71. (Perhaps Domes would say that they are returned to power today.) What these awkward terms seem to boil down to is that the first group wanted change toward a more popular and egalitarian government, while the second would allow the government structure to bureaucratize, as it did repeatedly. Although Domes warns that these orientations were not mutually exclusive, how substantially different is his polar analysis from the neater (though not necessarily more enlightening) radical versus moderate view?

Domes may have intended an advance of theory to be the book's main point. But an overloading and underweighing of data make it otherwise. The numerous tables on the ages, education, provincial origins, and professions of groups of leaders are

useful. But leaders are people, and the author's perceptions of human motives and behavior are shallow. He writes in the stilted styles established by official Chinese documents (dry as those from any bureaucratic culture), American political science jargon, and handouts from various governments. The hundreds of citations to provincial radio broadcasts are valuable for giving access to the local press that is otherwise forbidden to outsiders. But the citations are made without reference to published sources, leaving readers hard put to review his facts for further analysis. The book's lack of political sophistication, historical sensibility, and literary elegance will restrict its readers to the most dedicated China watchers, who will value it as the only book so far that doggedly mines data from these four years.

Domes's final words are typical of the book's confusing attitudes and maddening style: "But, in the long run, these contradictions may well lead to the final completion of the Chinese Revolution: China's definite entry into the modern world. *This* [his emphasis] revolution, however, may go beyond the capacity of Communist leadership. And in *that* long run, we may still not all be dead."

ROXANE WITKE
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SIMON LEYS. *The Chairman's New Clothes: Mao and the Cultural Revolution*. Translated by CAROL APPLE-YARD and PATRICK GOODE. New York: St. Martin's Press. 1977. Pp. 261. \$16.95.

The Chairman's New Clothes is an English translation of Simon Leys's 1971 French publication. The author, a Belgian specialist in Chinese art, argues that the Chinese Cultural Revolution was "a simple power struggle at the top" and that there was "nothing revolutionary about it except name, and nothing cultural about it except the initial tactical pretext." The organizational format of the book is chronological. After a brief introduction, the narration of events proceeds month by month under the title of "A Diary of the Cultural Revolution." The work closes with three short postscripts and an appendix that includes eight official documents as well as biographical sketches of major Cultural Revolution figures.

If Leys's sharp eye for Chinese art, his provocative writing style, and his critical perspective helped his previous travelogue—*Chinese Shadows*—reach the bestseller list, the same qualities prove utterly inappropriate for this subject. The Chinese Cultural Revolution was an extremely complex historical phenomenon. The author's approach reveals a simplistic thesis, selective data, and incoherent interpretation and explanation of events.

The book says nothing new or revealing about the Cultural Revolution. Clearly, the subject matter requires more than "impressions" conveyed through a sensational writing style.

More specifically, Leys writes randomly and uncritically. The book reads like the Red Guard publications he cites. Uncritical use of his source materials also makes the book lag not far behind a Red Guard publication. Following the Chinese tradition, the author is preoccupied with the name rather than the substance of the Cultural Revolution. Rhetoric replaces careful analysis, for example, when the author speaks of "Chiang Kuo-fa, a notorious butcher of Red Guards," or argues that "the red sun of Maoism is manifestly no more than a bloodstained, setting sun." In addition, he frequently resorts to the Red Guards' favorite technique: he uses materials that have no relevance to the basic points asserted, but uses them in a sensational fashion. On one occasion, the author offers his readers a full text of a speech by Mao, originally made to explain his view of the Cultural Revolution to foreign visitors. Leys includes the seven-page speech in his "diary" of August without any comment on Mao's view, obviously for no other reason than that Mao happened to make it in the same month. With little objective analysis and consistency, the book leaves readers with more mysteries and confusion than answers or insight.

The importance of the Cultural Revolution for understanding contemporary Chinese politics does not need any reiteration. If the complexity of its issues, events, and actors poses an exciting intellectual challenge, the seven-thousand pages of Red Guard publications available in the Western academic community offer an excellent research opportunity. A careful and systematic analysis of this vast information source, plus a coherent analytical framework, will illuminate such key questions as why Mao initiated the mass movement unprecedented in scale, what roles the various elite groups and the mass organizations played in it, why all the mass organizations split into the two warring factions, and how this polarization affected the dynamics of the movement. Unfortunately, this book neither raises nor answers any of these crucial questions.

HONG YUNG LEE
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PETER R. MOODY. *Opposition and Dissent in Contemporary China*. Stanford, Calif.: Hoover Institution Press, Stanford University. 1977. Pp. xiii, 342. \$14.95.

Peter R. Moody's *Opposition and Dissent in Contemporary China* is an important book because it disproves the oversimplified view of the People's Re-

public as a totalitarian state and at the same time questions the applicability of the interest group model to China. Moody describes China as a society with a totalitarian ideology but also one that has various kinds of opposition and dissent at the center as well as on the fringes. These movements are due less to the growth of interest groups than to the emergence of political factions.

There is ideological justification for opposition and dissent in Mao Tse-tung's theory on non-antagonistic contradictions, which specifies that differences between the leaders and the led should be brought out into the open. In fact, when these contradictions threaten the control of the Communist Party or Mao Tse-tung, they are suppressed. There is no institutional toleration nor legal protection for routinized opposition and open dissent as found in our society. Nevertheless, cracks appear in the totalitarian structure, particularly when a power struggle divides the leadership. As Moody points out, neither the party apparatus nor Mao is strong enough to make policy by imperial edict, so there is room for factional politicking among the elite. For example, a group of literary intellectuals whom Moody calls "Marxian Confucians" became associated with the party apparatus in the early 1960s. The apparatus allowed them to demand a return to more conventional Marxist practices, though it was less tolerant of their desire for Confucian humanist values. Another group of younger ideologues whom Moody calls "establishment radicals" clamored for renewed revolutionary spirit; they eventually joined with Mao to launch the Cultural Revolution.

When Moody moves beyond the elite opposition to sectoral opposition, where he has less information, he is less convincing. He describes the potential for opposition in such groups as the former landlords, peasants, workers, religious sects, and local interests, but he has few actual examples. He has more concrete material on educated youth and intellectuals and their spin-off into opposition movements on the fringes of society, but here again their opposition has less to do with their professions than with their ideological disagreements with the prevailing regime.

Some may fault Moody because he does not make a clear distinction between opposition and dissent. Opposition indicates an effort to rule in place of the existing rulers, whereas dissent implies protest against certain policies of the rulers, but not necessarily the desire to overthrow them. Nevertheless, as Moody shows, the distinction in China is blurred. Dissent provides the grounds for opposition movements and has led to the formation of groups and the formulation of programs that seek to change the rulers and, in some cases, even the system.

Because this book was written before the purge

of Mao's wife Chiang Ch'ing and her radical cronies, some of Moody's predictions have not materialized. Moody does not distinguish between the relative importance or unimportance of opposing groups on the fringes, though he does so for the center. Nor does he clearly analyze Mao's relationship to opposition at the center, a relationship that constantly changed. His book is peppered with clichés. But these are minor criticisms. Moody's balanced approach will help us re-examine the nature of China's totalitarian state and gain insight into the dynamics of change within China's ruling elite. Most important, it convincingly demonstrates that concern for human rights may be less legalistic and more ideological in China but is not unique to Western societies.

MERLE GOLDMAN
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GEORGE ELISON. *Deus Destroyed: The Image of Christianity in Early Modern Japan*. Cambridge: Harvard University Press. 1973. Pp. xiv, 342. \$25.00.

Less than a hundred years after its auspicious beginning in the mid-sixteenth century, the effort to Christianize Japan was a shambles, a victim of the Tokugawa centralization. As the period of the first encounter between Japan and the West, the "Christian century" has been the object of much scholarship. George Elison's *Deus Destroyed* is an outstanding contribution to the extensive literature in Western languages.

Rather than yet another narrative account, Elison provides the best treatment thus far of the religious and intellectual confrontation between Japan and the West. Most scholars outside Japan have tended to write from a Western or, frequently, a Christian point of view, but Elison succeeds in dealing with his subject from the inside. In assessing the Christian century within the context of early modern Japan, Elison concludes that its greatest influence was negative: that the rejection and denigration of Christianity served an important role in maintaining the policy of national seclusion and the socioreligious controls of the Tokugawa state. Although Elison's conclusion is by no means unique, he succeeds in treating the theme of the rejection of Christianity with more depth and sophistication than any previous work.

In part one, "Problems in the Acceptance of Christianity in Japan," Elison emphasizes the vulnerability of the Japanese mission from the very beginning, even before persecution began: The shortages of missionary personnel in Japan and the unwillingness of the Jesuits to train and use sufficient numbers of Japanese priests; the incompatibility of Japan's highly particularized feudal loyalties with Christianity's demands of a

higher loyalty to God; and, perhaps most crucial, the necessity of the missionaries to engage in political intrigues in order to gain a foothold for Christianity—intrigues that made inevitable the later indictment of the Christians.

The heart of Elison's work is part two, "Forms of the Rejection of Christianity in Japan," and the translations which follow in part three. As in the first part, Elison organizes his discussion around prominent figures rather than events. Outstanding among them and the subject of an entire chapter is Fabian Fucan. A Japanese convert who, as a Jesuit lay brother (*iruman*), was an effective advocate of Christianity among his people but later renounced the faith to become a formidable foe of the Christian movement, Fabian best represents for Elison the ultimate fate of Christianity in premodern Japan. Fabian possessed a remarkable intellect and holds the distinction of having written both the outstanding Christian tract (*Myōtei mondō* [1605]) and anti-Christian tract (*Haī Daiusu* [1620]) of the Christian century. In part three Elison translates the full text of the latter and borrows the title, which he translates as "Deus Destroyed," for his own volume. Elison's treatment of Fabian's writings and his elucidation of the peculiar relationship between the pro-Christian and anti-Christian works is masterful. It is unfortunate, however, that biographical materials on Fabian are rather scarce and that his personality and, above all, the basis of his decision to renounce Christianity, remain elusive.

Elison's painstakingly annotated translations reveal the diverse nature and role of anti-Christian writings. Fabian's knowledge of Christianity and his point-by-point rebuttal of its major doctrines in "Deus Destroyed" made the work useful to the authorities in their attempts to induce apostasy among Christians, including missionaries. Another work written specifically for this purpose was *Kengiroku* (translated by Elison as "Deceit Disclosed") by the Jesuit apostate Christovão Ferreira. The militant Buddhist perspective is represented by *Iha Kirishitan* ("Christians Countered") by Suzuki Shōsan, who personally preached Zen Buddhism in former Christian strongholds. Finally, the anonymous *Kirishitan Monogatari* gives a good impression of the sinister but nevertheless exotic view of Christianity that was directed toward the common masses and was evidently consumed with relish.

Elison is more than equal to the considerable linguistic, disciplinary, and intellectual demands of his subject. Like Fabian Fucan, product in part of a Jesuit education, Elison turns from Western to Eastern theology and philosophy with an ease reminiscent of Fabian himself. Elison has consulted an impressive number of primary sources and demonstrates his mastery of the voluminous

secondary literature (Western and Japanese) both in the body of his book and in his lengthy, erudite notes.

Deus Destroyed is, in sum, intellectual history of rare distinction, a work that illuminates many facets of a fascinating chapter in Japanese history and in the history of contact between East and West.

MICHAEL SOLOMON
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ANN WASWO. *Japanese Landlords: The Decline of a Rural Elite*. Berkeley and Los Angeles: University of California Press, for Center for Japanese and Korean Studies, University of California, Berkeley. 1977. Pp. viii, 152. \$10.00.

Japanese Landlords offers a critical re-evaluation of the economic and social roles of agricultural landlords in Japan (excluding Hokkaido) in the period from 1868 to 1941. The broad coverage is invaluable for its treatment of changes in landlord-tenant relations over time and from region to region. The shortcomings of this approach, the result of limited data, leave the reader wishing for more consistency in the foci of the analysis. Yet, despite the fragmentary documentation, Ann Waswo has assembled a remarkably coherent and intelligent analysis of landlord-tenant relations.

The book's introduction sets the context for the analysis, exploring the historiographic background in both Japan and the West. The author clarifies her concern with the "rice roots" rather than ideological issues, as well as the inconsistencies between stereotypical "landed elites" and Japanese landlords: landlords provided services while extracting rents, though this changed during the period considered. Chapter two, "Landlords and Landlord-Tenant Relations in the Meiji Era," discusses the impact of the Meiji land settlement and the environment in which landlord-tenant relations developed. The point is made that in Meiji Japan "tenants were more vulnerable to exploitation than ever before" (p. 21).

Chapter three, "Landlords and Agriculture," explores landlords' roles in rationalizing and improving cultivation. While self-interest may have been their major motive, landlords contributed to agricultural progress. As a consequence, "the vertical ties between landlords and tenants began to be replaced by horizontal ties among tenants themselves" (p. 65). Chapter four, "Changes in the Landlords' Role in Rural Society," treats the decrease in landlord-cultivators, their turn to non-agricultural involvements, and increased absentee ownership. All three trends were more apparent in economically advanced regions. "Landlord-Tenant Conflict" is the focus and title of chapter five.

From 1917 to 1931 disputes were concentrated in Kinki and Chubu, while from 1932 to 1941 Tohoku ranked first and conflicts in Kinki declined in number. Waswo describes the regional distribution and causes as well as the changing nature of landlord-tenant conflict. Tenant assertiveness was not the result of desperate poverty. The impact of the Depression on smaller landlords contributed substantially to tenancy disputes after 1931.

In her conclusions Waswo stresses that landlord promotion of agricultural improvements brought tenants increased economic security while reducing their dependence on landlords. Relations between the two groups became increasingly impersonal and strained. Landlords' involvements in nonagricultural affairs furthered tensions by dissociating them from rural life. By the end of the Pacific War most landlords accepted the need for major land reforms. Few, however, appreciated their role "in bringing about the deterioration of their status and influence in the countryside" (p. 139).

Japanese Landlords is a well-written and valuable addition to the social and economic history of modern Japan.

WILLIAM B. HAUSER
University of Rochester

GAIL LEE BERNSTEIN. *Japanese Marxist: A Portrait of Kawakami Hajime, 1879-1946*. (Harvard East Asian Studies, number 86.) Cambridge: Harvard University Press. 1976. Pp. xiv, 222. \$13.00.

This is a thoroughly researched intellectual biography of Kawakami Hajime, a renowned Japanese Marxist in the post-World War I period, who is best remembered for his attempt to reconcile Buddhist truth with the "scientific truth" of Marxism. Kawakami's life represents the kind of dilemma that confronted many Japanese intellectuals of the prewar years who had to cope with the problems of identity crisis resulting from Japan's rapid transformation into a modern society. To show the development of Kawakami's complex personality as well as to gain an insight into his inner life, Gail Lee Bernstein provides a broad frame of reference in which she traces Kawakami's gradual evolution toward Marxism—from patriotic nationalist to academic Marxist and, finally, to revolutionary Communist—in the political and social context of his time. The book seeks to answer several questions. Why was Kawakami attracted to Marxism? What was his understanding of Marxism? What solution did he see in Marxism to the social problems confronting Japanese society? And why did he hesitate to commit himself fully to Marxist teachings?

As the author points out, the dominant themes throughout Kawakami's intellectual life were "the conflicts between ethics and politics, religion and science, and, ultimately, Japan and the West" (p. xiii). This is understandably so, since, as the heir of a Chōshū samurai family, born just about a decade after the Meiji Restoration when Japan's modernization process was not yet in high gear, Kawakami naturally could not free himself from the influence of samurai ethics and the traditional Japanese values. This heritage also explains the slow process by which he was converted to Marxism and eventually joined the Japanese Communist party in 1932. A prominent professor of economics at Kyoto Imperial University, Kawakami was also a man of strong religious conviction, particularly in Zen and Pure Land Buddhism. His contention that Buddhist truth and Marxist truth are not necessarily in conflict, but could be complementary to each other in human life not only showed a remarkable Japanese adaptation of Marxist doctrine but also extricated Japanese Buddhism from the classic Marxian attack on religion as the "opiate of the masses."

In a larger context, Kawakami's attempt to modify Marxism represented a consistent effort by Japanese intellectuals to reconcile modern Western ideologies with Japanese traditions. This proclivity for syncretism was so common in prewar Japan that it can also be found in the case of Kawai Eijirō, a professor of economics at Tokyo Imperial University and a non-Marxist socialist, who endeavored to inject a Japanese version of ethics and idealism into socialism. In this regard, Kawakami was no exception among prewar Japanese intellectuals, who were characterized by general lack of originality of thought.

On the whole, the work is a brilliant analytical study of the life and thought of Kawakami Hajime. But by tracing the career of Kawakami one can also come away with a clear understanding of the nature and scope of left-wing politics in prewar Japan. The book has, no doubt, made a valuable contribution to the study of prewar Japanese Marxism.

B. WINSTON KAHN
Arizona State University

MICHAEL BLAKER. *Japanese International Negotiating Style*. (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1977. Pp. xviii, 253. \$15.00.

Will nothing be left for those who trade in international good-guy, bad-guy stereotypes? Latest to fall is that personification of sinister statecraft, the prewar Japanese diplomat. Michael Blaker lays

him to rest in four uneven but provocative chapters that add a new behavioral dimension to our understanding of how the Japanese conducted high-level negotiations in the half century of imperial haggling between Shimonoseki in 1895 and Pearl Harbor in 1941. Drawing heavily on Japanese sources, Blaker convincingly argues that Japan's reputation for deceit and double-dealing at the conference table is "unwarranted and undeserved" (p. 223). Japanese diplomats scorned underhanded methods and rarely departed from what they preached. Unfortunately, they operated within a ferocious system full of self-righteousness and other pressures that acted not only to hamstring them at every turn but ultimately to sully their own and the nation's high sense of honorable purpose.

This is but one of several intriguing conclusions that the author offers after sifting through eighteen major, more or less representative, instances of Japanese diplomacy in action. (All are strongly political; seven focus on Russia, although no Russian sources are used.) Underlying Japan's whole approach to the diplomatic arena was its conception of negotiation as "a profoundly face-threatening event" (p. 12). To minimize the risk of failure, incredibly painstaking preparations went into every negotiating encounter. Because of a byzantine decision-making apparatus, booby trapped everywhere with internal strife and other forms of "dissensus," formulation of demands, selection of negotiators, and so forth were not simple matters. When the nation's bargaining goals were at last set, they were viewed as inherently just and proper and open to little, if any, revision. Japanese diplomats, as a result, invariably found themselves in a "Great Bind," caught between a domestic audience demanding that they gain all and a diplomatic community expecting them to offer reasonable concessions. This was extraordinarily difficult for them to do. Japanese negotiators aimed not to strike a mutually acceptable bargain but to fight tenaciously for every last bit they had set out to achieve. Nor did Tokyo help much. Slow moving and often indecisive, it generally limited itself to exhorting its envoys to follow their instructions and try harder. Was the system successful? Blaker thinks in many ways Japanese negotiating style was exemplary. But in the end, as we all know, their mania for lopsided agreements wrangled in secret became self-defeating.

There is much in this book to commend, including the highly serviceable translations from Japanese documents (the account of the 1919 Peace Conference is especially valuable). There are some weak points, but none worth getting excited about. The author's episodic approach makes for repetition which, along with his flights to the conceptual

heights, becomes wearisome after a time. Annoying, too, is the lack of a comparative sense. Too many "Japanese" norms are really just the japanized version of traits common to statesmen everywhere. Diplomatic historians are also likely to be less surprised by his findings than Blaker—whose political-science chauvinism and neglect of some basic literature (for example, Hilary Conroy's "Government versus 'Patriot'") are very evident—seems to believe. His contribution, however, is impressive. It is almost certain to prove more enduring than the diplomatic achievements he analyzes so perceptively.

ROBERT J. GOWEN
East Carolina University

SANG-CHUL SUH. *Growth and Structural Changes in the Korean Economy, 1910-1940*. (Harvard East Asian Monographs, number 83.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press, Cambridge. 1978. Pp. xii, 227. \$15.00

Although the Japanese presence ended in August 1945, Sang-chul Suh's comprehensive survey of the pattern of growth and structural changes in the Korean economy of the Japanese colonial era covers the period from 1910 to 1940, which he divides into three distinctive subperiods. He characterizes each of the three periods as follows: (1) 1910-19, the period of preparation for effective control; (2) 1920-30, the period of reform and relaxation of control, promotion of agricultural economy, and complete economic integration; and (3) 1931-40, the period of rapid industrialization and the deemphasis on agricultural economy. His work is divided into nine chapters, including a short chapter on the historical background. The author's aims are to measure the rate of economic growth and to shed light on the implications of the economic change through a case study in which the economic theories of Simon Kuznets are examined.

Suh reduces the shortage of reading materials on the economic history of modern Korea, points out the unique features of the growth pattern of the Japanese period, and, through his excellent statistical study together with many tables (ninety-seven in all), helps the reader to see the pattern of change in the Korean economy. He successfully proves that, although the growth pattern of the Korean economy showed certain modern characteristics, the economic growth that the Japanese imposed upon Korea lacked "those spontaneous forces of growth in society" and dynamic factors that normally cause "chain effects" of a continuous economic development and changes in social values (p. 144). He points out that the basic pur-

pose of Japanese policy was to make the Korean economy a part of the periphery of the Japanese economy and to benefit the Japanese.

The author finds that the Japanese economic policy that promoted economic dualism in Korea along with economic growth and institutional changes brought no tangible benefits to the Koreans, who were "mobilized through coercive measures" (p. 144). Moreover, the modern industrialized economic sector was "irrelevant to the traditional sectors and the majority of inhabitants" (p. 145). He concludes that, while Japanese residents in Korea and some Koreans reaped the benefits of the modern economy, the traditional economic sectors, namely agriculture and the handicraft industries, suffered; unlike elsewhere, Korea saw no substantial degree of urbanization. In the labor field, he finds that real wages of the workers actually declined while the new economy created some 653,000 surplus workers.

He emphasizes the fact that, although the colonial economy enhanced the potentials of the Korean economy for rapid growth, the imposed economic growth that caused "a total exhaustion of both population and resources" together with the Japanese legacy made postliberation economic growth more difficult (p. 155). The bibliography includes works in English, Japanese, and Korean but almost no primary sources.

ANDREW C. NAHM
Western Michigan University

AHSAN RAZA KHAN. *Chieftains in the Mughal Empire during the Reign of Akbar*. Simla: Indian Institute of Advanced Study. 1977. Pp. xiv, 271. Rs. 45.

A number of important contributions on medieval India by the scholars of Aligarh Muslim University have considerably deepened our understanding of the structure of Mughal rule in India. Ahsan Raza Khan's monograph, emerging as it does from the Aligarh school, is yet another well-written addition to the Mughal "nobility" theme so diligently pursued by these scholars. Basing his work on the *Akbarnama* and the *Ain-i-Akbari*, Khan has systematically analyzed all of the available Persian and non-Persian sources to describe the autonomous Muslim and Hindu "chieftains" of India who accepted, or were often forced to accept, the overlordship of the great Mughal emperor Akbar.

With the aid of excellent maps, Khan gives exhaustive details about the chieftains, *subha* (province) by *subha*, and in doing so gives us insights into the actual political process of the conquest and consolidation of the regional powers by Akbar. As a reward for loyalty, Akbar evidently made some of the chieftains *mansabdars* (high-ranking of-

ficers) in the Mughal administration; others had to pay tributes (*peshkash*) or offer military services to sustain their autonomous rule. Akbar exercised direct control over the chieftains by such means as keeping hostages for good behavior and holding on to his prerogative to nominate the heir to the vacant throne of a dead or rebellious chieftain. It becomes clear from Khan's account that initially the chieftains would rally around the banner of their old regional overlords against Akbar and after the defeat of their lords, one by one, would submit to Akbar's rule. In a standard book on Akbar—and there are many—we find dramatic accounts of the great wars he fought to gain supremacy in India, but often the real nitty-gritty process of empire-building was carried on at the regional level and had to do largely with the submissions of the local chieftains. The author has succeeded admirably in giving us a step-by-step description of that slow but important process.

Khan correctly observes that the political organization of the chieftains, in many cases, was based on clans/clan brotherhoods (*ulus*) and that many chieftains were known by their clan affiliations. The kinship network of such clans, often militarily organized, sustained the power of a number of chieftains in almost every *subha*. The chieftains belonging to the great clans—such as Gakkhars of Lahore *subha*; Chaks of Kabul *subha*; Baloches, Samejas, and Dharejas of Sindh *subha*; Gahlots, Parmars, Kathes, Jaitwas, and Baghales of Gujrat *subha*; Rathods, Kaccahvas, Chauhans, and Sisodias of Ajmer *subha*—had the required political clout with which Akbar had to come to terms. Khan has appended a useful list of chiefs who were granted *mansabs* by Akbar. He has also given the details of matrimonial alliances Akbar and his family had with the daughters of the chieftains. The appendixes confirm Khan's assessment that the policy of religious toleration initiated by Akbar was, among other things, a kind of accommodation to the political expediency of winning over the loyalty of the powerful chieftains.

An area of research to which Khan could have addressed himself concerns the status of the chieftains vis-à-vis the other loyal supporters of Akbar coming from the military and administrative ranks. I would like to know, for instance, about the role of the chieftains in the court politics of Akbar. Another neglected area is the nature of the relationship that the chieftains had with their own subordinate chieftains (intermediary *zamindars* and primary *zamindars*, to use N. Hasan's words). But these are minor observations, and they do not in any way diminish the merit of this painstaking and original work.

N. K. WAGLE
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B. B. MISRA. *The Bureaucracy in India: An Historical Analysis of Development up to 1947*. New York: Oxford University Press. 1977. Pp. xii, 421. \$13.25.

Bureaucracy is an especially important subject for study in modern India in view of the government's dominant role (and the weakness of private business) in economic development. Before independence, the most successful Indian university graduates sought places in the civil services, bureaucrats often were able to command the largest dowries, and administration provided lifetime employment to a disproportionate share of the educated classes. B. B. Misra, with earlier observers, believes that the administrative "steel frame," because of its efficiency and probity, was Great Britain's finest creation in South Asia.

The focus of this book is much narrower than the title suggests. It neglects the subordinate and specialized services and concentrates largely on the internal affairs of the Indian Civil Service, the thousand-man administrative service recruited exclusively in England until 1922. Misra is interested in how ICS members were selected, trained, and promoted. We learn what law, political economy, and history books candidates were assigned but not how their contents may have affected their values and behavior. Similarly, we learn that between 1858 and 1897, 21 percent of all ICS recruits were the sons of clergymen and 17 percent were the sons of merchants and manufacturers, yet the significance of these figures is not explored. Misra devotes special attention to the process and implications of the Indianization of the ICS. By 1922, Indians held 15 percent of ICS posts; by 1939, when the last examination in London was given, Indians held half.

Misra's admiration of the ICS, apart from its racial exclusion of Indians, is apparent throughout. In his view it approached Max Weber's ideal type of bureaucratic organization in its systematic, rational, impersonal, specialized operations. Only with the coming of democracy did the ICS come under pressure to move away from its previous impartiality. He produces information from the papers of Sir Malcolm Hailey to suggest that after the election of Indian provincial ministries in 1937, Congress ministers, including Morarji Desai in Gujarat, attempted to use the civil service for partisan purposes. Misra seems to deplore the effect of Indian democracy on bureaucracy, although he cites Max Weber on the value of representative government as a check on bureaucratic absolutism. He contrasts unfavorably the rising Indian politicians with the ICS. Out of the irrationality and emotionalism of Indian politics, he says, emerged "a tiny minority of power elites who . . . had little in common with the Indian

masses" (p. 376) and who lacked the administrative skills, education, and honesty of the ICS officers. The ICS, in contrast, had been "recruited from a layer of society known for its intelligence and industry, education and respectability" (p. 392). Unfortunately, the analysis of the social background of Indian bureaucrats and of the subordination of the ICS to elected ministers is impressionistic and fragmentary and it ends on the eve of World War II, not with the transfer of power in 1947.

Persons interested in how ICS officers viewed their role in India, the functions they performed, or the efficiency and impact of their performance will be disappointed by this book. They are likely to find the works of L. S. S. O'Malley, Philip Woodruff, and Bradford Spangenberg more useful. Misra's book, however, will be valuable to those seeking information on the recruitment, preparation, and Indianization of the ICS.

JOHN R. MCLANE
Northwestern University

ERIC STOKES. *The Peasant and the Raj: Studies in Agrarian Society and Peasant Rebellion in Colonial India*. (Cambridge South Asian Studies, number 23.) New York: Cambridge University Press. 1978. Pp. viii, 308. \$27.50.

"To probe the nature of the 'traditional' agrarian order in which the vast bulk of the population of the Indian subcontinent was embraced, and to explore the extent to which rural society underwent fundamental alteration under colonial rule" (p. 1), states the introduction, are the unifying threads in this collection of twelve previously published essays. Even to readers familiar with them, however, this volume needs no justification, because Eric Stokes's ideas and arguments continue to stimulate and provoke thought long after they have been digested once or twice. Furthermore, strung together, they convey nicely the author's intellectual progression and thematic concerns over the last decade.

These articles are also important as essays in critical demolition. Stokes carries on an insightful dialogue not only with contemporary observers of colonial India, such as Baden-Powell, Mill, Maine, Marx, and Weber, but also with present-day writers. Although his onslaughts against facile generalizations and simplistic analyses are almost always on target, they appear at times to be particularly sharp toward American scholarship. As ever, his writing spouts energy.

The opening essay sets the tone by raising the basic problems and questions to be confronted in the volume. It begins with a caveat against the

simple polarities of nineteenth-century sociological thought, "tradition and modernity, continuity and change, status and contract, feudalism and capitalism, caste and class" (p. 19), which persist in the historical consciousness about South Asia. That these stark dichotomies colored the imagination of the contemporary British mind also serves as a reminder of the disparities between official ideology and Indian realities, a point reinforced by the fourth essay, "The Land-Revenue Systems of the North-Western Provinces and Bombay Decan, 1830-1880."

Stokes's picture of the changes wrought in the first century of British rule is both complex and subtle. As he concludes in the opening contribution, "the village was given external peace and high, unremitting taxation, conditions which created a major problem of adjustment for the village elite. The period saw the slow decline of warrior castes and the enhanced prosperity of thrifty, agricultural castes, the expansion of cultivation, the extension of cash-cropping, the gradual fragmentation and fractionalisation of land-holdings, and a tighter dependence on the money-lender-graintrader" (p. 45). These processes are highlighted again in the essays "Privileged Land Tenure in Village India in the Early Nineteenth Century," "Agrarian Society and Pax Britannica in Northern India in the Early Nineteenth Century," and "Peasants, Moneylenders and Colonial Rule." They also figure prominently in his analyses of "The Structure of Landholding in Uttar Pradesh, 1860-1948" and "Dynamism and Enervation in North Indian Agriculture," which shift the focus to the second century of British rule.

Yet "none of the processes went far. It was the upper levels of society that took the full shock of conquest" (p. 45). Precisely such an effect, however, as his seminal studies of both the general context of the "Mutiny" Revolt of 1857 and its local histories indicate, was the critical factor in the differential rural response to this rebellion.

All the essays bear out Stokes's final reflection that the historian need no longer follow the analytical frameworks of the sociologist and the economist because he is now in a position to generate his own.

ANAND A. YANG
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RAGHAVAN N. IYER. *The Moral and Political Thought of Mahatma Gandhi*. Reprint. New York: Oxford University Press. 1978. Pp. xiii, 449. Paper \$4.95.

Historians of modern India will welcome this soft-cover issue of a landmark study, first published in 1973. The title hardly does justice to the scope and

depth of Raghavan N. Iyer's work, which embraces the entire framework of the Mahatma's ideology. Stressing the concepts of *satya* ("truth") and *ahimsa* ("noninjury"), he probes the meaning and implications of Gandhi's theories in the context of such European writers as Rousseau, Hume, Locke, and Marx and in the light of Vedantic, Buddhist, and Jain texts.

Iyer's chapter "Swaraj and Swadeshi" presents Gandhi's working philosophy with a clarity to be found in no other source. Some may, perhaps, question his statement that "at the center of Indian political thought lay the concept of *swaraj* or self-rule . . ." (p. 347). One looks in vain for the word "*swaraj*" (so dear to Indian nationalists) in Ghoshal's standard *A History of Indian Political Ideas* and in many of the classics of Hindu political thought. The term is used in a rather restricted sense in the Vedic literature. The *Aitareya Brahmana* refers to *swarajya* simply as a form of government peculiar to western India. Nevertheless, in the Gandhian sense of "rule over one's self," as against mere political independence or republican form, the concept, if not the actual term *swaraj*, is indeed basic to the Hindu political tradition and is proclaimed as essential to successful government by Manu, Sukra, and even the Machiavellian Kautilya. One might say it is *dharma* ("duty, law, virtue") that is actually "the center of Indian political thought," but Iyer is well aware that, for Gandhi, *dharma* itself is not attainable except through individual *swaraj*.

So thorough is Iyer's mastery of both Western and Eastern philosophical systems and his understanding of specific theorists that one is persuaded to accept the book's theses with few challenges. One curious thesis, however, is that "several Indian mystics, like Ramana Maharshi, thought that Gandhi . . . sacrificed his spiritual development by taking on too great, too Atlas-like a burden upon himself" (p. 380). Elsewhere Iyer states that Ramana Maharshi thought Gandhi should "retire into the seclusion of an anchorite" (p. 212). The author does not identify the other "Indian mystics" or give any source for his statements. But in the major publications of Ramana Maharshi, including three volumes of his recorded dialogues, there is nothing to support such a contention. On the contrary, speaking in 1938 to Rajendra Prasad, and later to a delegation of congressmen, he endorsed Gandhi's political activism. Some months before the assassination he remarked, "If Gandhi keeps quiet, leaving aside all his activities, people will ask, 'why is he keeping quiet?' He must do what he has come for."

The Moral and Political Thought of Mahatma Gandhi will remain a classic. It is unlikely that any future writer will bring the historical knowledge, philo-

sophical insights, and personal contracts required to approach the brilliance and depth of this study. Among innumerable others, this is the single most useful publication for an understanding of the mind of Mohandas K. Gandhi.

D. MACKENZIE BROWN
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Santa Barbara

CLIVE S. KESSLER. *Islam and Politics in a Malay State: Kelantan, 1838-1969*. Ithaca, N.Y.: Cornell University Press. 1978. Pp. 274. \$15.00.

This is a most impressive and useful book. It is impressive both in what it attempts and in what it achieves, though in fact the two do not quite coincide. It is useful both to those interested in Malaysia—because of the light which it throws upon affairs in one of the crucial states in that country—and to those interested in the promise and the difficulty of serious interdisciplinary work—because of the skill, imagination, and mixed results with which the author takes and joins elements from different fields in order to formulate his thesis.

Kelantan was, in three key ways, a kind of odd state out on the Malay Peninsula during the period considered in *Islam and Politics*. First, there was Kelantan's geography. It follows the Malay-state pattern of encompassing the drainage area of a river system. But where the Malay populations in other states were often sparse and scattered, the Kelantanese, because their river opened out onto an alluvial plain some miles from the sea, were able to construct the type of settlement dominant in other places in Southeast Asia but rare in peninsular Malaysia: an extensive (some six hundred square miles), densely populated (over 500,000 people) area devoted to rice agriculture. Second, relatively isolated as it was in northeast Malaya, Kelantan did not partake extensively in the massive exporting of extracted products and importing of immigrant populations that characterized economic and social developments in other Malay states during the British colonial period. Third and signally, Kelantan's voters, after Malaysian independence, turned down, and continued to turn down, the Alliance Party that dominated politics at the national level and in the other peninsular states in Malaysia, opting instead for a state government led by the opposition Islamic party. The standard explanation for this political aberration stresses Kelantan's isolation and its backwardness, citing the religious extremism and anti-Chinese feelings of the electorate as manifestations of those qualities. Clive S. Kessler, however, has attempted to go deeper into the soul of Kelantan

and has in the process turned up strong evidence that voting patterns there, and the feelings and interests that determine them, are far more intimately connected with Kelantan's unique agricultural base and with the kind of society that that base supports than with Kelantan's isolation or backwardness.

Basing his view on extended periods of close anthropological research in one Kelantan township and focusing it through a sharp review of Kelantan history and a penetrating essay on the sociology (and, therefore, fitness as political platform) of Islam, Kessler sees Kelantanese peasants voting the Islamic party both because through the party they could speak for their class interests and because in Islam they possessed a powerful way of seeing themselves and their complex world. To Kessler, Islamic party supporters were not acting out of negative emotional impulses. If their votes were negative, they cast them against aristocratic manipulation, against landlordism, against corruption. On the whole, however, their votes were a positive, distinctly political expression, reflecting a fairly clear perception of their position in the socioeconomic situation in Kelantan and of the moral principles and consequences connected with their political choice.

This is a compelling view, but there are problems with it and with both the whole and the parts of the argument from which it derives. The historical sections that deal with the township are brisk and innovative, but those on the state as a whole represent selective choices from, rather than any substantive additions to, previous knowledge of Kelantan's history. This in itself is understandable, but it does mean that where historians to date have failed Kessler—in not providing information about the Japanese occupation, or in not looking closely at Kelantan's economic history, for instance—the development of his argument is weakened. During his field work, Kessler was unable to gather full information about land ownership; his acknowledgement of this makes it no less serious a shortcoming as he attempts to distinguish class structure in a largely agrarian society. Though much of Kessler's analysis of the sociology of Islam is conducted at a fairly general and fairly sophisticated level, an understanding of the applicability of that analysis to the actual situation in Kelantan rests largely on the poignant comments and observations of a very few Kelantanese; this can be a thin thread with which to tie together two such bulky packages. The three parts of the book—the Kelantan/historical, the township/anthropological, and the Islamic/sociological—fit together well, but not perfectly. Nagging doubts remain. Is the line between the township and Kelantan always clear? Are all the historical influ-

ences, and not only the more useful ones, given proper weight? Would the framework used in the book to bind together spiritual and material interests make as much sense to Kelantanese as it does to readers?

Perhaps the most important question is largely untreated: whether the views and convictions of that large minority of Kelantanese, many of them also peasants, who did not support the Islamic party differ in nature from, or are as valid and sincere as, those of the people who supported it? This is a question that has grown in significance since the book was written. In March of 1978, state elections in Kelantan whisked the Islamic party from power there, apparently leaving the state open to the kind of development projects and loose money that the Islamic party's supporters had wished to forestall. At issue now in Kelantan is not only whether political life is undergoing marked and permanent change but also whether the shape of agrarian society and the depth of Islamic conviction are also to be altered. It is a mark of the quality of Kessler's book, despite its problems, that clearly these are in some fashion intrinsically united in Kelantan.

WILLIAM J. O'MALLEY
Cornell University

ABDUL AZIZ ISHAK. *Special Guest: The Detention in Malaysia of an Ex-Cabinet Minister*. (Oxford in Asia Historical Memoirs.) New York: Oxford University Press. 1977. Pp. x, 210. \$24.95.

Modern Malay political biographies have been devoted to major establishment figures; autobiography has been the genre of the mavericks, including the unusually prolific Abdul Aziz Ishak. Two previous volumes (in Malay) traced his genteel origins, his employment in the colonial civil service, his attraction into anti-aristocratic nationalist circles, his family's association (as both journalists and proprietors) with the leading Malay newspaper *Utusan Melayu*, and his role in the decolonization process and subsequently as a minister in the early governments of independent Malaya.

A populist among aristocrats, an impetuous individualist among careful men wary of unpredictable enthusiasms, Aziz Ishak fell out with his political associates in 1961–62. His resignation as minister of agriculture followed closely upon his attempt (recounted in Karl Von Vorys's *Democracy without Consensus*) to create cooperatively owned fertilizer factories, for which he sought to rally popular support against both his sceptical cabinet colleagues and the large multinationals ICI and Esso. After drifting into opposition ranks, Aziz was accused of treasonous involvement in the In-

donesian confrontation against Malaysia (a charge outlined in the 1965 white paper *A Plot Exposed*), detained without trial for one year, and kept under close surveillance and restraint for several more.

As a rejoinder to the white paper, *Special Guest* is too imprecise and cryptic to be really convincing (a complaint perhaps anticipated by Aziz's plea that he speaks not as much as he would, but only so much as he dares). Nor does it provide a sharp account of the life of a Malaysian political detainee (as, for example, do the fictionalized works of Ahmad Boestamam). More concerned to extol the solace afforded a detainee by renewed religious faith, it provides no original homiletics either. On all of these counts this memoir disappoints the reader (as well as its author's hopes for vindication) because of a single failing: rambling and disjointed, replete with trivial details but assuming great familiarity with the context, it sorely evidences a lack of needed copyediting. It also lacks the scholarly introduction and notes with which (according to the dustjacket) all volumes in this Oxford in Asia Historical Memoirs series are provided. Regrettably, these deficiencies will limit the book's appeal to specialists, even though it contains material of potentially wider interest—most notably about human rights issues in Southeast Asia, but also about its engaging author with his hybrid regimen of Quran and golf.

Although the circumstances surrounding his detention still remain obscure, many of the causes Aziz Ishak espoused—high-yielding rice, technical education, closer relations with the USSR and Afro-Asian countries, and greater political and economic attention to the Malay peasantry—have meanwhile become official policy. By reminding his critics of that fact, *Special Guest* may yet help rehabilitate its author's political reputation.

CLIVE S. KESSLER
Barnard College

C. M. TURNBULL. *A History of Singapore, 1819-1975*. New York: Oxford University Press. 1977. Pp. xvi, 384. \$36.50.

It was C. M. Turnbull's intention to write "a sympathetic personal interpretation" (p. xv) of Singapore's history. The author indeed presents social, economic, and cultural information selectively. In addition, this chronicle caters to the specialist in political, administrative, and military history. The work opens with a discussion of all precolonial settlements of record that had existed on the island. Subsequently, Raffles's prescience, perseverance, and planning, along with Farquhar's assistance, led to the establishment of the colonial city. Starting with the residency of John

Crawford, the book treats the nineteenth-century history of Singapore, for the most part, in the traditional manner, by governors' periods in office. Commerce had primacy in Singapore's life and conditioned almost every governmental action. Population growth and ethnic composition were influenced by the importation of Chinese and Indian labor. As early as 1827 the Malays had declined in population to third place. Bugis, Arabs, Baweanese, Armenese, Armenians, Europeans, and Jews made up the minorities. The author notes early ethnic occupational specializations, but does not describe the degree to which later generations were able to enter new fields in the twentieth century. The sexual imbalance of early nineteenth century Singapore encouraged the Chinese secret societies' traffic in women and the institution of the notorious *mui tsai* adoption system, whereby Singapore was provided with female domestic servants and prostitutes under conditions of near slavery. Turnbull, however, fails to mention the role of other women, Malays and Eurasians, in molding Singapore's, as much as Straits societies', traditional culture, since males other than Indian Muslims intermarried.

By the early twentieth century, Chinese society was divided between the locally born and recent immigrants from China and between the English- and Chinese-educated. While Turnbull discusses the backing Singapore Chinese gave to the Chinese Revolution, she ignores the dialect group loyalties that vitiated unified support. The anticolonial stance of the Chinese community, as well as their anti-Japanese boycotts, also are related to events in China. British underestimation of Asian competence facilitated General Yamashita's brilliant invasion bluff. The tragic consequences of the Japanese occupation fell particularly hard on the Chinese community.

The book's account of the first decade after the Japanese surrender concentrates on Chinese student activism in relation to Communist successes in China and on the Malayan Emergency. The political maneuvers of the most important political parties are discussed at length, as is the success of political leadership in bringing about internal self-government in 1957 and in conducting the delicate negotiations with Malaya resulting in the integration of Singapore into Malaysia from 1963 until its ouster in 1965. Lee Kuan Yew's tactics in removing the radicals from leadership of the People's Action Party (PAP) are recounted, as well as the PAP's success in securing the allegiance of the Chinese-educated masses. But the local structure of the PAP's bases of support is not discussed. Nor does the author reveal the internal dynamics of Singaporean Chinese society in the twentieth century that propelled Lee Kuan Yew

and other brilliant English-educated technocrats to the leadership of an independent Singapore. For that matter, there is little information about the antecedents of Singapore's leaders from other ethnic groups. An additional anomaly is the lack of mention of a single distinguished woman by name during the entire course of Singapore's history.

Some information is presented on the effective economic development planning that enabled Singapore to industrialize and survive the setback of British military withdrawal and the building up of competing free trade entrepôts in Malaya and Indonesia. Short paragraphs describe Singapore's major social accomplishments: the dramatic reduction in fertility, the building of large-scale housing estates, the expansion of primary and technical education, and the distribution of essential amenities to a large proportion of Singapore's population. These developments might have been described in greater detail since Singapore's major achievements, apart from economic survival, have been in social engineering. By the 1970s social welfare benefits have been cut, and the attraction of capital investment has become more important than implementing socialist reforms. Increasing government regulation, while beneficial, has changed Singapore from a free society into a guided democracy.

Turnbull has synthesized an enormous range of material from many fields into the most comprehensive and valuable history of Singapore written to date. The book is enriched by an extensive annotated bibliography. A map of the urbanized sector of Singapore, within twentieth-century city boundaries, would have been useful as well for understanding patterns of expansion.

PAULINE D. MILONE
Palo Alto, California

KARL D. JACKSON and LUCIAN W. PYE, editors. *Political Power and Communications in Indonesia*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xxi, 424. \$18.50.

Assisted by a grant from the research office of the USIA to MIT's Center for International Studies, the editors of this volume have brought together thirteen chapter-length studies by eight Americans, two Indonesians, and one Australian. Four chapters, ranging from a formulation of a "theoretical framework" for the Indonesian bureaucratic state and its future, to analyses of "patron-client relations" and of political culture in Indonesia generally, are by Karl D. Jackson. Curiously, there is no chapter by the other editor, Lucian W. Pye.

Basic themes of some chapters (for example,

those on the Indonesia military by Ulf Sundhausen, on economic policy by Bruce Glassburner, and on the politics of Indonesian Islam by Allan A. Samson) have already been dealt with in varying degrees in previous publications by the same authors. Occasionally a chapter, because of its comprehensive compilation of known data, has become a useful "state of the art" reference piece. This is the case, for example, with the chapter, "The Mass Communications System in Indonesia," by Astrid Susanto, dean of the Journalism Faculty of Bandung's Padjajaran University. Even so, Susanto's conclusions, such as the dominant role of newspapers as sources of information and the need for a distinctive "media mixture" in terms of Indonesia's highly varied audiences, are hardly surprising. Perhaps only one chapter in the book, Benedict Anderson's informative account of "cartoons and monuments" as communication channels under the present Suharto regime, breaks new ground, illuminating as it does the persistence of traditionalism in elite political attitudes. There is a rather selective bibliography, referring to text citations, with a generous sprinkling of sources in Indonesian.

A failure to communicate clearly what its various chapter analyses add up to is, perhaps, the book's principal disadvantage. Despite the high quality of some individual contributions, it seems to have been difficult to be specific about a common theme among them, except, perhaps, at a very high and uninspiring level of abstraction. Moreover, the reasons why a particular topic of "communications" in relation to the Indonesian "polity" was chosen for treatment and others were not are not apparent. To contend, for example, as the preface has it, that "there have been remarkably enduring qualities in Indonesian politics," that change from the Sukarno to the present Suharto era is not as deep as might be imagined, that the authors included here are neither particularly optimistic nor very pessimistic about Indonesia's course, and that the real "forces of change" that are at work lie beneath the "surface drama" of recent years, is to box the compass with unchallenging platitudes that could discourage all but the less informed reader from going on in these pages.

The thematic problem is hardly improved by Jackson's three-page, highly debatable, concluding summary on "the prospects" for the Indonesian bureaucratic polity. In this he contends, for example, that neither Suharto nor his "bureaucratic elite" has much impact on the daily lives of Indonesian citizens. This is one of those half truths that *pari passu* could be argued for most, if not all, of the "bureaucratic polities" in the world today. It is well to affirm again (as virtually all students of modern Indonesia have done prior to the appear-

ance of this volume) the continuity of the country's religious values and social structures, despite changes in regime in the capital. But surely, contrary to what Jackson contends, the significant changes brought by the Suharto era—for example, the collapse of the Indonesian Communist Party as a principal power center or the accelerating pace of industrial development—have very much had an “impact on the daily lives of most Indonesian citizens.”

“Only if we revive and sustain scholarly research will we be able in time to understand the processes of change and continuity in a society as complex as Indonesia's,” the preface of this volume asserts. The present reviewer, not being aware that scholarly research on Indonesia had recently collapsed, would suggest that the present volume is solid proof that scholarly research continues to be very much alive and that, as in the past, it can always use some improvement.

JUSTUS M. VAN DER KROEF
University of Bridgeport

E. H. MCCORMICK. *Omai: Pacific Envoy*. New York: Oxford University Press. 1977. Pp. xviii, 364. \$33.00.

James Cook made three spectacular voyages between 1768 and 1779, sailing farther and for longer periods than any other explorer-adventurer. Through his voyages light was shed on many geographic mysteries. An accomplishment of his which is not readily remembered is that, on his second voyage, he brought back Omai of Huahine, who became the first South Sea islander to visit England and return safely to his home.

E. H. McCormick has written a lengthy and sometimes belabored account of Omai (or Mai as some called him), who impressed and charmed British society of the time. The work is exhaustively researched, and no one could hope to learn much more than McCormick shares with us, not only of Omai, but also of Cook, the Pacific, and the era of the voyages. He goes far beyond mere description and travelogue to comment on the science and philosophy of the times that motivated Cook and others to collect people from new lands.

Some people have an image of early European Pacific explorers meandering through the vast expanse, stopping occasionally at islands (chatting, trading, and, sometimes, fighting with the natives), reprovisioning, and then sailing on to other emerald lagoons and white sand beaches. Such was not the case. Most of the voyages were carefully planned (even if the captains were not too sure where they were going), and many contacts were made with the island peoples. Rousseau's

notions of the “noble savage” promoted great interest and curiosity about islanders among Europeans. They were thought of as “pure” people, unfettered by civilization.

Omai was about twenty-one when he arrived in England on July 14, 1774. He was placed under the charge of Joseph Banks—entrepreneur, financier of ships' voyages, and veteran of Cook's first trip—who, together with his friends, “launched him on a meteoric social career” (p. 95). Omai was popular at once in London. “The newspapers . . . vied with one another to follow his every movement. . .” (p. 97). At the University of Cambridge “the Doctors and Professors struck him wonderfully” (p. 116). His activities calendar was crammed and probably did not allow him much focus. During one week, reportedly, he heard the king speak, went to the theater, attended the House of Lords for the opening of Parliament, and called upon the scholar Dr. Burney and his family. If Omai was a noble savage when he arrived, McCormick's account leaves little doubt that he was, if not dissipated, at least appropriately bewildered by the time he departed.

Captain Cook returned Omai to his home islands in 1777 and there set him up with a European-style house, livestock, gardens, and a number of other western accouterments largely useless on Huahine. Soon Omai discarded these things and became once again a true Tahitian.

McCormick intended to write a definitive account, and this he did. Scholars of the Pacific who want to know about Omai, Cook, and their times will have to consult it, for much of the material has not been published before. The author has also assembled all important portraits and sketches of Omai, together with a number of illustrative drawings. Beautifully done and well arranged, they enhance the value of this volume.

DIRK A. BALLENDORF
College of Micronesia

A. T. YARWOOD. *Samuel Marsden: The Great Survivor*. Melbourne: Melbourne University Press; distributed by ISBS, Forest Grove, Oreg. 1977. Pp. xv, 341. \$26.00.

The Reverend Samuel Marsden has been an enigma to historians. Some have called him a religious hypocrite and a flogging magistrate; others consider him an important founding father of Australia, a true missionary to the New Zealand Maori and to the Pacific Islanders, and a farmer who so successfully experimented with the breeding of fine merino wool that he died a wealthy man. A. T. Yarwood refuses to paint a black and white picture, but skillfully, and at times brilliantly, brings out the greys. Marsden emerges

as a successful though ruthless businessman and as a parson who could not escape "the zealot's great error of believing that ends justified the means" (p. 281).

Marsden's humble origins are clearly emphasized. He worked as a blacksmith until age twenty-five when he entered Cambridge University, financed by a grant from the Elland Clerical Society, which supported poor but zealous men for the Anglican clergy. Wilberforce encouraged his selection in 1793 as an assistant chaplain to New South Wales, and he continued his chaplaincy until his death in 1838. From 1800 to 1825 Marsden functioned alone as the chaplain of the Anglican church. He was also an agent for the Church Missionary Society and the London Missionary Society.

Yarwood has refused to ignore the ignoble in Marsden, particularly his work as a minister. How could he succeed as a chaplain, Yarwood asks, if he passionately preached to the convicts, yet refused to associate with ex-convicts? Yarwood has clarified many questionable aspects of Marsden's long and contentious career, among them the following: (1) the charge that Marsden was intolerant of dissenters is disproved, yet he was a bigot towards Roman Catholics (more for political than religious reasons); (2) he was ill served by Governor Lachlan Macquarie (1810-21) and denounced far too harshly by biographer M. H. Ellis (1947); (3) Yarwood questions Professor Manning Clark's accusation in the first volume of his *History of Australia* (1962) that Marsden's sermons were bereft of charity and the love of God, and far too filled with fire and brimstone; (4) Yarwood admires Marsden's toughness and adaptability enough to subtitle his biography "The Great Survivor"; (5) Marsden was a man of enormous courage; (6) he was really dedicated to the protection of the colony's children born of convict parents, and; (7) he was devoted to the distant Maori, yet rejected the Australian aborigine, mainly because the latter was nomadic and less cultivated than the former. The aborigines were also a threat to his many pastoral lands.

The book is based on painstaking research in Australia, New Zealand, and England. Marsden emerges from his diary and letters, as well as from newspapers and government dispatches and other primary sources, as a magistrate of unnecessary harshness, a priest with many failings, a farmer whose wealth derived from convict labor, a public figure who unscrupulously attacked anyone who opposed him, and a politician who was devious, calculating, and at times untruthful. On the positive side, he was a happy family man, devoted to his ailing wife and sturdy children (though he did not get along very well with his son). He was a

man of principle some of the time, though annoyingly self-righteous all of the time. He was also a psychologically complicated personality; but Yarwood is far too wise and sophisticated a historian to attempt a psychohistorical interpretation when the evidence is only partial at best.

Yarwood's greatest contribution is to paint a picture of Australia's early history and to explain Marsden's life in the context of political, social, and religious events of an often sordid penal colony struggling toward free immigration and self-government. In so doing Yarwood proves himself to be a truly first-class historian.

SAMUEL CLYDE MCCULLOCH
University of California,
Irvine

P. LOVEDAY *et al.*, editors. *The Emergence of the Australian Party System*. Sydney: Hale and Iremonger. 1977. Pp. xviii, 536. Cloth \$19.95, paper \$9.95.

This is the first comprehensive effort to deal systematically with the origins of the Australian party system. The contributors are among the most competent specialists available. The scholarship is of high order, drawing on a variety of original sources as well as secondary literature. Separate attention is given to party emergence in the six Australian colonies (later states) and at the national level in the early years of federation. There is an overview chapter at the beginning, and a long, integrative essay at the end.

The resulting product is, by and large, commendable. Australianists will clearly profit from the detailed, colonial-state and federal accounts. Some of the previous, at times diffused, literature on politics at large is reassembled to conform with the party-political thrust; a number of insights are themselves fresh. A wider readership, both among historians and political scientists, will look to lessons that derive from this formative period in Australian politics.

The several contributors do not superimpose a common analytic framework on their writing, and readers must often make their own mental notes as to what accounted for similar or divergent party growth patterns in the colonies-states. P. Loveday's concluding essay reviews some of the comparative and theoretical literature on party origins generally but makes no concerted effort to force the Australian experience into a larger mold of party theory. What is especially useful is his recasting of the individual essays into working hypotheses about Australian politics. There is the movement from a nonparty stage to party life quite fully articulated, rather than from middle-class to mass parties. There is a consideration of the

forces—such as economic development, large-scale strikes, a major depression, and federation itself—that impelled the crystallization of parties. This eventually led to a realignment along Labor versus non-Labor lines, even though Labor versus non-Labor was not the invariable, original pattern in the colonies. There also is an attempt to explain why, on the basis of very early tendencies, the “Liberal” dimension of politics acquired a distinctive reformist, innovative feature, while Labor did not become doctrinaire. There is an assessment of the conjunction of programmatic congealment and of organizational complexity as parts of the party-building effort. Throughout, one of the services rendered by the contributors has been their attentiveness toward the non-Labor aspects of party development, despite the frequent tendency in the literature to overemphasize Labor as the centerpiece.

The basic outlines of the Australian party system were in place by 1910, the point at which the book’s narrative concludes. How the party system looks and behaves now is a reminder of the considerable continuities within the Australian political community. This book suggests how a combination of fundamental and fortuitous factors set in motion the process by which the modern party system emerged, and laid its deep foundations.

HENRY S. ALBINSKI

Pennsylvania State University

HENRY S. ALBINSKI. *Australian External Policy under Labor: Content, Process and The National Debate*. Vancouver: University of British Columbia Press, 1977. Pp. xi, 373. \$22.00.

The scope of this study is comprehensive, including diplomatic relations, trade, defense policy, and immigration. It analyzes the period from the victory of the Labor Party in December 1972 to the overturning of the government by the Liberal-Country Party coalition in December 1975. The principal sources include newspapers, political party records, and interviews with party leaders, members of parliament, ministerial advisers, and diplomats. Since the official diplomatic records of this period are closed, the quality of the study depends largely on the information gained through interviews and the judgment of the author in using this information. Most of the interviews were on a nonattribution basis, and this placed a heavy responsibility on the author. It can be said unreservedly that in handling this responsibility Henry S. Albinski has left nothing to be desired. He has shown not only that in a period when diplomatic records are closed an enormous amount of inside information can be ferreted out, but also that evi-

dence gained through interviews can be dealt with in a skillful, judicious, and fair manner. Throughout the study the analysis is perceptive and the interpretations and conclusions are persuasive.

Albinski sees both change and continuity in external policy under the Australian Labor Party. From the outset the ALP sought to make independence the linchpin of overseas policy and to avoid what it regarded as the excessive dependence on the United States practiced by the preceding L-CP ministries. The impulsive and outspoken Labor prime minister, E. G. Whitlam, exercised a dominant role, and he made it clear that the ANZUS pact with the United States and New Zealand was not the “be-all” and “end-all” of Australian external policy. He dispatched a letter of complaint to Nixon concerning the bombing of North Vietnam, and his government granted enthusiastic recognition to Communist China, North Korea, and North Vietnam. Moreover, the government’s emphasis on anticolonialism and anti-racialism brought the cutting off of wheat exports to Rhodesia and an Australian vote in the United Nations for expulsion of South Africa from that world organization. These and other actions by the government brought accusations by the L-CP opposition that Labor was talking a smooth line about evenhandedness while fawning over Communist and Third World states. Yet with all of this, Labor never contemplated breaking the ANZUS connection, and, on many practical issues, such as the operation of the American naval communication station at North-West Cape, it continued to cooperate closely with the United States. No serious damage was done to United States-Australian relations, both because American leaders and diplomats avoided getting too excited about ALP rhetoric and because American foreign policy was also changing—though less markedly—in a way that paralleled the Australian change under Labor.

Most historians would probably prefer that this study be less topical in organization, and some, perhaps, would like less detail. But these possible criticisms are insignificant when weighed against the merit of this excellent book.

RAYMOND A. ESTIUS

Tulane University

UNITED STATES

J. R. POLE. *The Pursuit of Equality in American History*. (Jefferson Memorial Lectures.) Berkeley and Los Angeles: University of California Press, 1978. Pp. xv, 380. \$14.95.

This study addresses itself to the discrepancy between the public commitment to equality in America and the translation of that commitment into policy. It is not an in-depth study of the Constitution or of constitutional law, nor is it an exploration of untapped and unfamiliar evidence. Rather, it is a brilliant interpretation of transforming ideas and of the relationship of those ideas to social structures and politico-legal policies. Some of these ideas J. R. Pole has considered in a previous monograph. What gives an added dimension to this study, however, is that it embodies the point of view of an extremely knowledgeable British scholar who is confessedly fascinated by the operation of egalitarian principles in a nation like the United States, with its federal structure and pluralist values. A wide-ranging interpretation, both in time and subject matter, Pole's book constitutes a thoughtful and provocative contribution to the continuing and intense debate on a central theme in the American democratic tradition.

The author sets in a historical framework some six categories: (1) equality before the law; (2) equality of opportunity; (3) equality of esteem; (4) political equality (or equality of power); (5) sexual equality; and (6) religious and moral equality. Pole attributes the tenacity with which the idea of equality has persisted in America in no small measure to the historical structure of American institutions, to the formal and constitutional origins of the American nation, and to the fact that equality has entered into the language of justice more explicitly than in most contemporaneous systems.

It is in the area of ideas that Pole's book will probably prove most useful. Equally at home with the Whig and Jeffersonian traditions, with Jacksonianism, with the views of Reconstruction, with Social Darwinism, and with such recent philosophic and sociological approaches as offered by John Rawls and James S. Coleman, the author is less original and less penetrating in his interpretation of key constitutional cases dealing with equality. For the subject of equal protection, with its manifold ramifications, the reader will still need to consult the work of constitutional historians and legal scholars, for example, Laurence H. Tribe's *American Constitutional Law: A Structure for Liberty*.

Possibly least persuasive is Pole's description of church and state relations as stemming from a claim for equal treatment between warring sects. Instead, it can be argued that the purpose behind the establishment clause in the Constitution was to assure that church and state do not unite to create the dangers implicit in such a union and that the intention behind the free exercise clause was to assure that government does not excessively intrude upon religious liberty.

In his concluding chapter, "An Incomplete Rev-

olution," the author poses the present dilemmas that confront American society. As he shows, equality of opportunity is not wholly compatible with equality of esteem, and the conflict currently rages between an egalitarian ideology and the accepted American system of incentives, with its stress on individual liberty. Likewise, he recognizes that a policy of imposing an absolute equality of results would, even if attainable, represent a substantial departure from all previous concepts of equality of opportunity. This tension between equality and individual liberty is recognized in the author's concluding observation that, although the Constitution's normal gravitation pulls in the direction of equalization, it is the individual whose rights are the object of special solicitude.

RICHARD B. MORRIS
Columbia University

WILLIAM B. SCOTT. *In Pursuit of Happiness: American Conceptions of Property from the Seventeenth to the Twentieth Century*. Bloomington: Indiana University Press, 1977. Pp. xi, 244. \$12.50.

This book offers a nicely sustained but necessarily impressionistic and sketchy survey of the idea of property in America. William B. Scott first traces the creation of "the agrarian republic," in which the theories of Harrington and Locke, transformed by the New World environment, gave rise to the fundamental American conception with its elements of natural right, individual proprietorship, free enterprise, and equal access to productive resources, especially land. The advance of industrial capitalism in the nineteenth century altered this conception. The South had its peculiar problem, of course, while in the North the earnings of wage labor, with the consumer goods those earnings bought, took on the values earlier attached to proprietorship. In the law, at the same time, corporate privileges became virtually indistinguishable from the original natural rights of property. Part 3, "The Collectivist Age," begins with a perceptive discussion of Justice Field's use of the "due process" clause of the Fourteenth Amendment to protect business enterprise from the application of the police powers of the state, then characterizes four American collectivists (Bellamy, Veblen, Croly, and Dewey), and concludes with the efforts of "neo-Jeffersonians" (for example, the Southern Agrarians) to recover the moral values associated with the individualistic tradition of property holding in America. Scott takes note of the remarkable persistence of the Jeffersonian ideal despite the loss of its substance, but he does not examine the meaning and implications of this gap between reality and belief.

The method of the book is perfectly straightforward. It is, essentially, to trace the idea of property through selected thinkers: "those individuals who gave some thought to the concept of ownership and whose thought was either significant or particularly perceptive." With such a standard it is difficult to determine who should be included and who excluded; nor is it clear on what basis Scott has actually made these decisions. Any knowledgeable reader will want to add or substitute names for those in the book—names no less important to the history of the idea of property and which, if included, would change the story somewhat. (On this reader's list are Raymond, Shaw, Patten, Berle, and Arnold.) Another problem with this approach is that it treats the subject, "conceptions of property," in the abstract, divorced from institutions (in Veblen's sense) and from public policies, which are the embodiments of ideas. Although property is an immensely practical subject, Scott rarely relates ideas to public policies, issues, and debates. Of course, it may be said that he does not have the space for this; indeed, as it is, with only two hundred pages of text, his tendency is to treat his thinkers symbolically and to treat their ideas as images.

The cast of characters and the ideas are familiar, but Scott offers many fresh insights as well as a coherent interpretation of the idea of property in America through several generations. This is no small achievement. Perhaps the most significant, though scarcely original, statement made by the book is the title. It asserts that the Jeffersonian ideal of "the pursuit of happiness" was bound up with a conception of property. This is certainly true; but Scott is not very clear on the matter, and readers will be misled if they assume that the Jeffersonian ideal encompassed nothing more.

MERRILL D. PETERSON
University of Virginia

JUDY BARRETT LITOFF. *American Midwives: 1860 to the Present*. (Contributions in Medical History, number 1.) Westport, Conn.: Greenwood Press, 1978. Pp. xi, 197. \$15.95.

The number of births attended by midwives in the United States declined from roughly 50 to 15 percent between 1900 and 1930. Medical reformers of the era campaigned for the elimination of lay practitioners and argued that the advance of obstetrics depended on medical monopoly of the fees and clinical experience provided by childbirth. The rise of operative obstetrics and popular demand for painless delivery provided physicians with a rationale for defining pregnancy as a disease rather than as a normal biological process. By the early

1970s over 99 percent of American births were delivered by physicians in hospitals, but significant numbers of women were questioning the organization of childbirth and rediscovering an older tradition in which the female midwife turned to the physician only in exceptional cases.

American Midwives provides a balanced narrative of the twentieth-century debate over the role of the midwife in childbirth. Judy Barrett Litoff makes clear the ambiguity surrounding the issue of education and licensing for midwives. Among the alarming discoveries of Progressives were the high American infant and maternal mortality rates. While admitting that midwives were no more responsible for this phenomenon than general practitioners, medical leaders argued that the elimination of midwives was a key to improving the status of physicians and that better births would follow. A minority of medical spokespersons pointed to European successes with trained midwives and the need for obstetrical care that the poor could afford. Most of the support for the trained midwife came from public health officials and others who were particularly aware of the social responsibilities of physicians, while critics tended to be advocates of private practice and narrow professionalism.

The strength of Litoff's study is her even-handed presentation of a wide spectrum of opinion. *American Midwives* meshes well with several recent studies: Jean Donnison's *Midwives and Medical Men*, a study of the midwifery controversy in Great Britain; Jane Donegan's *Women and Men Midwives*, which focuses on the nineteenth-century American debate over man midwifery; Richard and Dorothy Wertz, *Lying-In: A History of Childbirth in America*, which does not cover much of the material analyzed by Litoff. *American Midwives* should not be the last study of this topic. Based almost completely on published sources, it does not exploit the records of organizations such as New York's Maternity Center Association or available oral histories of key figures. Litoff might also be faulted for failing to relate her story to other social events. For example, the fall in infant mortality between 1910 and 1916 is attributed to activities of the Children's Bureau (p. 54) during a period when the purity of water supplies and other changes in the standard of living were certainly major determinants of infant mortality. Nevertheless, *American Midwives* advances our understanding of the politics of childbirth in American society and will provide an accurate introduction for future investigators.

JAMES REED
Rutgers University

PETER H. LINDERT. *Fertility and Scarcity in America*. Princeton: Princeton University Press, 1978. Pp. xi, 395. \$22.50.

The history of American childbearing patterns is a complex story. It begins with the remarkably high birth rates that sooner or later became prevalent in the colonial period. Then, between the late eighteenth and early twentieth centuries, births declined among almost every segment of the American population. Since the middle of the 1930s, when the long-term decline came to an end, fertility has gone up and down abruptly and unpredictably. Although no historian or demographer has offered an explanation that covers the entire story, a number of recent studies have provided valuable insights on parts of it. This book is a welcome addition to literature on the history of fertility, for it attempts to solve some of the more perplexing problems of recent fertility patterns, whereas much of the other current work has focused on eighteenth- and nineteenth-century childbearing.

Peter H. Lindert has made a number of important contributions, but most historians will probably not find this book to be an essential item on their reading list. The book is heavily weighted toward technical and theoretical discussions that only a few specialists will either care about or comprehend. Only the fifth chapter, "American Fertility Patterns since the Civil War," and a section of the seventh chapter on trends in wealth and income inequality since the seventeenth century would have interest for most historians. The fifth chapter is a revision of Lindert's contribution to Ronald D. Lee, ed., *Population Patterns in the Past* (1977).

For those whose interests are specialized enough, this book is valuable for what it adds to our understanding of the complex interactions between economics and fertility. The first contribution involves the construction of an elaborate model of the process by which couples assess the cost of having an additional child at various stages of their life together. Especially interesting is the conclusion that couples rely heavily on experience (including that of their own childhood) in deciding how many children they can afford. As presented by Lindert, childbearing is the result of a rational process of weighing objective economic factors in the light of one's preferences. The second part of the book looks at how fertility trends have contributed to inequalities (not scarcities, as the title suggests) in wealth and income over the past century. Lindert argues that when fertility declines so too does inequality, for reasons that range from internal family dynamics through support for schools to the overall supply of labor.

Any book that undertakes to solve major historical problems will have flaws, and this is no exception. Much of the data Lindert uses to construct his theories and explanations about the history of fertility come from after World War II.

Economists, of whom Lindert is one, may find this process of projecting the present backwards comfortable, but historians are likely to wish for more evidence from the periods actually under study. I found Lindert's arguments convincing for the twentieth century, when widespread fertility control made assessments of a family's economic future more believable as the primary reason for having fewer or more children. Less clear is how well his theories explain eighteenth- and early nineteenth-century childbearing patterns. Possibly these theories do, but they need further testing. Similarly, the interaction between fertility and inequality is far more evident in recent years than previously. Lindert admits and demonstrates the importance of immigration on the labor force, wages, and inequality in the nineteenth century. In fact, his data show that, in spite of declining fertility at this time, inequality increased because of migration patterns. Perhaps most bothersome of all is the assumption that couples make decisions about childbearing on the basis of consultation about future economic well-being. Historians of the family have recently demonstrated the remarkable lack of communication between husbands and wives in the past. It is possible that Lindert's theories would fit one spouse, but not both. Women, for example, have often had strong reasons for wanting to have fewer births which have nothing to do with wage rates. Lindert also introduces the idea of life-time earnings into his model of couples' calculations, knowledge that he can estimate but that husbands and wives in their 20s and 30s may never have even considered, let alone been able to compute.

In conclusion, specialists in American demographic history will want to read this book, but most other historians will probably want to wait until the ideas contained here have been tested further and assimilated into the broader contours of the history of American childbearing.

ROBERT V. WELLS
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C. GREGORY CRAMPTON. *The Zuni of Cibola*. Salt Lake City: University of Utah Press. 1977. Pp. 201. \$15.00.

Living in several pueblos in the mesa country of western New Mexico, the Zuni Indians have always played an important role in southwestern history. Since the time of their first encounter with the Spanish conquistadors, the Zunis have been frequently visited by white missionaries and travelers who desired to convert them to Christianity or to beg and barter for food. Unlike other tribes who succumbed to the advance of European culture,

the Zunis adjusted to newcomers and still retained their Indian identity. European-oriented historians have only written peripherally about these people while focusing on Indian-white relations in the Southwest. Thus, a study which concentrates directly on this people's history has been long overdue. C. Gregory Crampton's *The Zunis of Cibola* will delight most general readers, but scholars will continue to wait for a thorough treatment of the tribe.

Despite the author's sympathetic approach (the book is dedicated "To the Zuni People"), the Indians never quite emerge from a traditional historical framework which describes them in relation to white contacts. Still, Crampton's effort is worthwhile, providing a good outline of the four-hundred-year relationship among Zunis, Spaniards, Mexicans, and Americans.

Part of the author's dilemma in writing the Zuni story stemmed from inadequate research. "Insofar as the sources would permit, I have used them to bring out the Zuni side of things, to catch the Zuni perspective on the Zunis' own past . . ." (p. xii), writes Crampton in his preface. But by ignoring all unpublished primary manuscript material, the author was compelled to produce a history that viewed the Zuni past from an outsider's vantage point. Thus, Zuni history becomes a series of clashes and visitations by non-Indians (certainly a desirable compilation but not tribal history). Which events do the Zunis regard as milestones in their past? Who are the heroes and villains of this tribe? What is the Zuni view of man's place in the universe? Such questions form the basis of a people's history, but in this account they remain both unasked and unanswered. Consistently, the names of Zuni leaders are ignored.

In addition, the oversimplification of a complex story has resulted in distortions. For example, when Fra Francisco Letrado was killed at Hawikuh on February 22, 1632, the author writes: "The possibility exists that Sunday mass may have coincided with a Zuni ceremonial . . ." (p. 33) and precipitated the murder. Continuing to speculate, Crampton suggests that Estevan the Moor was killed in 1539 under similar circumstances. Conflicts between Indians and priests (or conquerors) were actually far more complicated than a poorly timed arrival or the conjunction of a Sunday mass with a more traditional ceremony. It was not the Zuni gods who were jealous and demanding of monotheistic fidelity. Crampton later views the Great Pueblo Revolt of 1680 as a power struggle between Indian shaman and missionary priest. A more positive motive for the shaman might suggest that the Indians had concluded that the priests were practicing witchcraft. Crampton's outline of Zuni-white relations breaks off around 1900. Con-

sequently, the twentieth century, which has witnessed the most intense impact of alien culture at Zuni, has been largely ignored.

Despite these criticisms, the book is worth the price and should be considered by every person interested in southwestern tribes. Crampton is a beautiful stylist, writing with sincere feeling and knowledge about his native land, and the selection of photographs is superb. This book provides a good basic introduction to its subject. Perhaps someone will now be prompted to write a dissertation or scholarly monograph on the Zunis and utilize the extensive records available in Spanish, Mexican, and American sources, as well as in the Zuni oral history.

GERALD THOMPSON
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HENRI VAN DER ZEE and BARBARA VAN DER ZEE. *A Sweet and Alien Land: The Story of Dutch New York*. New York: Viking Press. 1978. Pp. xx, 560. \$19.95.

A descendant of Dutch New Yorkers once remarked at a conference several years ago that "the story of New Netherland is one of the best-kept secrets in American history." There is some irony in this filiopietist complaint because English New York as a colony long suffered greater historiographical neglect than its Dutch predecessor. Edmund B. O'Callaghan's two-volume *History of New Netherland* (1845-48) was the first in a series of affectionate accounts of the Dutch settlements along the Hudson, all intended to rectify Washington Irving's caricature of the New World Dutch as simple, stupid, contentious, and bibulous. In place of Diedrich Knickerbocker's fictionalized Dutchmen, these correctives created their own stereotypical New Amsterdammers—originators of all things American ranging from cookies and bowling to unblemished domesticity and public education.

Serious study of the Dutch experience in America has always been inhibited by the paucity of sources and the inability of most American historians to handle the Dutch language. The revival of interest in the historiography of the Middle Colonies during the past two decades has led to a minor renaissance of scholarly writing on New Netherland. The appearance of six volumes of Dutch historical manuscripts, originally translated by A. J. F. Van Laer and more recently edited by Kenneth Scott and Kenn Stryker-Rodda (*New York Historical Manuscripts, Dutch* [1974, 1976]) has made available fresh sources of information. In the vanguard of newer historians of the Dutch experience are Alice Kenney, Thomas Condon, Van Cleef Bachman, George Smith, and Ellis Raesly. Henri

and Barbara van der Zee, a husband-and-wife team of journalists, do not fall in this category. Although they utilize Dutch sources with which they are at ease, they nevertheless draw largely on familiar English materials, such as Jameson's *Narratives of New Netherland*, Fernow's *Records of New Amsterdam*, and O'Callaghan's *Documents Relative to the Colonial History of the State of New York*. And what they have written is traditional, narrative history. As such, it is surely the most voluminous account of New Netherland to appear in a century. It ranges widely over the American and European horizon, informing us in almost tedious detail of every quarrel between the directors of the colony and their subjects, between New Netherland and its English and Indian neighbors, and between England and Holland. One looks in vain, however, for some analysis of the role of women in New Netherland, the evolution and status of slavery under Dutch rule, the reception and development of Roman-Dutch law, land tenure, or family life and child rearing.

The authors' conclusion that the Dutch surrendered New Netherland without ever realizing the "fantastic prize" they possessed in the New World is hardly as significant as the fact that, when New York began its history as an English province, it was established on the basis of the easy-going tolerance, aggressive commercialism, and broad ethnic diversity that was the Dutch legacy. Indeed, for the Lords Directors of the Dutch West India Company, New Netherland was never so much a prize as a gamble—that the colony would produce short-run profits from the fur trade which would make unnecessary long-term investment in an agricultural settlement. But the van der Zees are less interested in analysis than in narration, and, however detailed, the narrative is neither very revealing in interpretation nor original in content. In the long run, then, this latest history of New Netherland is an interesting but not a very important book.

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PHILIP GREVEN. *The Protestant Temperament: Patterns of Child-Rearing, Religious Experience, and the Self in Early America*. New York: Alfred A. Knopf. 1977. Pp. xiv, 431. \$15.00.

The thesis of *The Protestant Temperament* is more easily summarized than believed. Philip Greven suggests that three temperaments—evangelical, moderate, and genteel—coexisted in America between the seventeenth century and the early nineteenth century. The differences among these three

orientations to life, especially between the evangelical and moderate, were often subtle, but, argues Greven, they were also profoundly important.

Evangelicals made breaking the infant's will the central task of child rearing. Although the method of attaining this end was not detailed in prescriptive literature, shame and guilt, not the rod, were probably most often employed. Fear and submissiveness toward parents prepared the child in these authoritarian families for his later total submission to God in the experience of religious conversion. Lacking a secure sense of identity, the evangelicals suppressed their rage. Occasionally, however, their anger erupted in attacks on ministerial or political authority. In delineating the evangelical temperament Greven relies on the prescriptive and personal writings of such figures as John Robinson, Cotton Mather, John Wesley and his mother, Susanna, and nineteenth-century Baptist educator Francis Wayland.

Moderates believed in limited authority in family, polity, and theology. Love and duty, not love and fear, were dominant child-rearing themes in these authoritarian families. Emphasizing the freedom of the will, moderates experienced gradual religious conversions. Moderates, exemplified by such leaders as John Winthrop, John Adams, Benjamin Franklin, and Thomas Jefferson, sought to control passion with reason.

Genteel parents tended to indulge their children. Affection solidified ties within the family circle, and children exhibited love and reverence toward their parents. Although punishment of children of genteel families tended to be corporal, particularly in the South, discipline was executed by parental surrogates. As adults the genteel were non-introspective, pleasure-seeking, and contented. They worshipped their God without much emotion. William Byrd, Thomas Hutchinson, and George Washington are the best-known examples of the genteel temperament.

Greven suggests that these three temperaments were formed in differing family structures and social milieus. Almost by definition, genteel households included nurses, servants, and other non-kin. Typically located in long-settled communities, moderate households included or were close to kin, especially grandparents. Evangelicals preferred the authoritarian intimacy of the nuclear unit. They feared the corrupting effects of servants and grandparents. Evangelical families may have been more geographically mobile and less economically successful than moderate families.

In his epilogue Greven draws connections between the temperaments and attitudes toward the ideology of the American Revolution. Each group used the same words, but each emphasized different meanings of the shared terms. The genteel

were pragmatic revolutionaries who, with Edmund Burke, defended liberty as an integral part of a wider set of values and institutions. Moderates focused more directly on liberty, emphasizing both individual rights and civic virtue. Finally, evangelicals perceived American liberty as communal; they rejected the self and self-interest and favored the creation of a "Christian Sparta."

Greven notes that the three temperaments are only a scheme of classification. Individuals exhibited more complex and variable orientations than can be captured by ideal types. Although the author concentrated on the evolution of ideas in his collection of sources, *Child-Rearing Concepts, 1628-1861* (1973), he omits a discussion of change in this volume. Although it is easy to overstate the extent of change in such matters as child rearing, time does explain the Calvinistic tenor of John Locke's "moderate" *Some Thoughts Concerning Education* and the derisive newspaper attack on Wayland's smug description in 1831 of how he broke the will of his fifteen-month-old son.

Greven has narrow intentions in this book. The linkage of temperament to family and social structure is thinly and unconvincingly documented. The prescriptive warnings of evangelicals against over-indulgent grandparents are juxtaposed to the close personal relationships of grandparents with the families of several moderates. The relationship of temperament to political ideology is not carried much beyond Alan Heimert's elusive discussion in *Religion and the American Mind from the Great Awakening to the Revolution* (1966). Greven cryptically notes that, "while I remain diametrically opposed to some of (Heimert's) central propositions, I also share many of his basic assumptions" (p. 11). Heimert's study, some critics argued, raised the spectre of a non- or trans-evidential mode of historical analysis. Read psychologically, historical documents can have limitless meanings. Greven rarely indulges in the loose style of psychohistory; "the very intensity of early Quaker testimony for peace suggests equally intense feelings of anger and aggression" (p. 115) is an exceptional departure from the usual rules of historical inference. Greven fails to address consciously the severe methodological and empirical difficulties of sustaining any proposition about temperament in the past. *The Protestant Temperament* is a richly suggestive contribution to cultural history. It is not, however, persuasive. Happily, Greven promises in future work to merge the intellectual themes in this volume with the social history of the family, which he pioneered in his study of Andover, Massachusetts.

DANIEL SCOTT SMITH
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DAVID E. STANNARD. *The Puritan Way of Death: A Study in Religion, Culture, and Social Change*. New York: Oxford University Press. 1977. Pp. xii, 236. \$11.95.

How can the historian do justice to a work so relevant to his colleagues, yet written for the reader learned in sociological theory, a book so plainly within the scope of this journal, yet one which begins with Peking man and ends in a modern hospital ward? At best he can restrict himself to matters within his competence; the rest he must leave to others. Moreover, the author's constant references to other writers is disconcerting. On every few pages we find phrases such as "Ariès has argued" (p. 46) or "C. C. Goen has noted" (p. 74); indeed, on four consecutive pages we see the Grays denying, Rutman showing, Morgan tracing, Ahlstrom contending, and assorted sociologists finding, terming, observing, cogently suggesting, concluding, and writing (pp. 135-38). The presentation and interpretation of facts rarely mix well with a bibliographical essay. But these difficulties, and a minor factual error about the English prayer book, cannot obscure the real merit of this book.

David E. Stannard's principal thesis, ably supported by the evidence, is that Puritan teachings inevitably created a conflict between two views of death. The pilgrimage motif encouraged the saint to regard death as the gate to everlasting life; thus Increase Mather proclaimed, "How glad should I be, if I might dye before I stir out of this pulpit" (p. 79). But the saint had to doubt his predestination, and on his death bed the same divine suffered intense fears of eternal torment. "An incongruity between an optimistic rhetorical interpretation . . . and a desperate introspective fear" (p. 146) was the hallmark of the Puritan view of death. With eternal fate sealed at death, there was no purpose in requiems or other religious rites, and, as the Puritan did not identify his physical body with his resurrection body, there was no merit in preserving his remains. Burials were unavoidable, but funerals were unknown in early Puritanism.

The simultaneous emergence of the funeral sermon and the jeremiad in the middle of the seventeenth century was no coincidence. As the first generation of saints departed this life or returned to England, as worldly interests claimed more attention, and as it became obvious that Cromwell was not the herald of the millenium, these homiletical genres served to remind the people of the loss of saints and to discourage them from identifying with an increasingly secular society. The unendurable tension between the accepted view of death and the existential experience of it became obsolete after the Great Awakening. Following Edwin Gaustad, Stannard sees the Awakening as the end

of Puritanism. The social structure had changed; more gradually, religious beliefs had deteriorated. The Awakening brought only a superficial and transient revival of old beliefs. In the author's pungent words, "the orthodox Puritan edifice . . . collapsed and brought down with it the unique Puritan tension between death and dying" (p. 154). The cemeteries reflected this change: on the grave-stones, "the grim visage of death had metamorphosed into a pleasant cherubic image" (p. 157). Henceforth—at least until relatively recent times—people were taught to look forward to death.

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SUNG BOK KIM. *Landlord and Tenant in Colonial New York: Manorial Society, 1664–1775*. Chapel Hill: University of North Carolina Press, for Institute of Early American History and Culture, Williamsburg, Va. 1978. Pp. xxii, 456. \$19.00.

One of the more revealing stories concerning European colonization is that of a certain Mr. Peel, who reached Swan River in western Australia with £50,000 and 3000 English workers but soon found himself without a servant to make his bed or fetch him water. "Unhappy Mr. Peel," Marx remarked, "who provided for everything except the export of English relations of production to Swan River!" Visionaries who projected feudal structures into the settlements of British America suffered fates rather like Mr. Peel's. Try as they might, manorialism, peasantries, and traditional landlord-tenant relations simply failed to root. New York, as Sung Bok Kim's valuable study makes clear, was no exception. Landlords acquired patents with feudal privileges and imposing titles, but these remained mere paper pretensions. The reality behind the feudal facade was the landlords' ability to exploit a corrupt political system to acquire several million acres of New York's best land and then to withhold that land from the market. By 1776, their success had produced at least six to seven thousand tenant farmers, a population unrivaled by any other colony in North America. The study of this process—of the origins of the great estates, of the careers of landlords and tenants and the structure of their relationships, and, more broadly, of New York agrarian society in areas where tenancy prevailed—is the purpose of this book. The result, based on detailed investigation of four of the largest and best-documented landholdings, greatly extends our knowledge of colonial social history and substantially revises much received wisdom on the New York land system.

It took a long time for the great estates to return substantial rental incomes. The land was initially

too vast and the tenants too few, forcing landlords to offer generous terms and earn profits from sources other than rents. During the first half of the eighteenth century, tenants breaking new ground could command secure tenures, rent-free periods of ten years or more, provisions for the first year, loans of the capital needed to begin a farm, and equity in improvements. Landlords turned short-term profits through interest on loans, through retailing, and by milling and marketing the crops of their tenants; they gained in the long-run through the improvement and peopling of their estates. The system was well suited to the development of a new region and everyone, Kim argues, prospered. Over time, however, the growth of population and increases in the value of land shifted the advantage from tenant to lord. The great landowners, once developers who combined estate management with diverse commercial activities, retired to their properties to live off the rents. The landless, content to lease when rents were low and terms favorable, became increasingly resentful of landlords who frustrated their desire to become freeholders and grew rich in the bargain. The consequence was intense, often violent, social conflict that began in the 1750s and was not finally resolved until the mid-nineteenth century.

It is a fascinating story, and Kim tells it well. Not that the book is without flaws. In particular, Kim's effort to defend the landlords from their contemporaries and his predecessors and to present a favorable view of tenancy results in missed opportunities and some special pleading. This is most evident in his discussion of the post-1750 disturbances, which are treated as almost entirely the consequence of unruly New England intruders and the imperial designs of Massachusetts, despite clear evidence of class, cultural, and ethnic conflict. Still, this is an important book, well written, detailed, persuasively argued, and firmly rooted in evidence—one that makes a lasting contribution to the social history of British America.

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DON HIGGINBOTHAM, editor. *Reconsiderations on the Revolutionary War: Selected Essays*. (Contributions in Military History, number 14.) Westport, Conn.: Greenwood Press. 1978. Pp. x, 217. \$16.95.

This collection of the papers delivered at the West Point symposium on the American Revolutionary War in April 1976 offers a provocative overview of the key questions relating to military aspects of the war. All nine essays, along with an afterword by the editor, are fresh in interpretation and suggest important areas for further study.

Two of the authors compare the Revolution to other uprisings of the eighteenth century. William H. McNeill finds similarities among the revolutions in America, Brazil, Corsica, and Russia: all originated from opposition to imperial administration, and none were revolutions for export. Peter Paret discovers little relationship between the military thought and practice in the American Revolution and that in three European conflicts.

Ira D. Graber examines the books read by the British generals and argues that they inherited an ambiguous strategic tradition: Caesar's aggressive flexibility versus Saxe's avoidance of confrontation. In America, however, the British had to experiment with a variety of strategies. Russell F. Weigley calls for more study of American strategy as it was and not, as so many historians have seen it, from the confines of Napoleonic doctrine. He critically reviews works that have treated strategy during the Revolution. It was a strategic defensive that Washington used, Weigley asserts, rather than a strategy of offense. Like Weigley, R. Arthur Bowler recapitulates a thesis from his own major work and argues that the chief reason for the ineffectiveness of a British offensive in America was logistical—particularly problems of forage. The English Treasury was inept in adapting to the changes in supply necessitated by the American war.

Don Higginbotham and John Shy delve into the role of the militia. Shy contends that the use of the militia aggravated social distinctions and affected political life in large measure. Higginbotham holds that the militia performed admirably and cites areas in which they contributed to winning the war. Richard H. Kohn discusses the civilian-military conflict—its causes and effects. Richard Buel, Jr. argues that time worked against the Americans; with the war grinding down to a stalemate and with American financial and supply problems during the later years, the British would have had the advantage if they had once again seized the initiative.

The essays are sound and annotation is complete. Perhaps it is pretentious to say that this reviewer found only one major factual error—the statement in one of the essays that Generals Weedon and Muhlenberg resigned during the course of the war. For the student and teacher of the American Revolution this book is especially valuable as a concise survey and as a reassessment of who fought and why and how they did.

HARRY M. WARD
University of Richmond

HARRY M. WARD and HAROLD E. GREER, JR. *Richmond during the Revolution, 1775-83*. Charlottesville: University Press of Virginia, for the Richmond

Independence Bicentennial Commission. 1977. Pp. xi, 205. \$10.95.

One senses that Harry M. Ward and Harold E. Greer, Jr. may have had to operate within certain constraints in writing this volume since they refer in the introduction to the abridgment of the manuscript to one-third of its original size. The result is a very brief account of less than two hundred pages of text; it is more descriptive than analytical, and there are no footnotes. It is nonetheless clear, particularly from the extensive bibliography, that the authors have examined the available sources thoroughly, and their brevity does not sacrifice detail. Every significant aspect of the Revolution as it affected Richmond—military, logistical, political, economic, and social—receives its due. Termination of the account with the formal end of war in 1783, it is true, leaves the story somewhat unfinished; immediately after the process of removing the state capital to Richmond began in 1780, the British campaigns in Virginia during 1781 brought dislocations, from which the little city had by no means recovered in 1783. Consequently, the community had yet to experience the full economic and political transformation that the Revolution ultimately brought.

Neither the rather abrupt end of the book nor its lack of a strong interpretative thrust, however, prevents the reader from gaining some vivid impressions of the impact of the Revolution on a community such as Richmond. Perhaps the most striking is a sense of the turbulence and dislocation that the Revolution wrought, especially during the final years of war. Richmond suffered serious destruction when British forces under Benedict Arnold invaded in January 1781 and additional damages at the hands of Cornwallis the following June. The further difficulty of organizing effective military support for the Revolution by either the state government or the Richmond community itself seemed to compound the problem. Conditions were almost worse in 1782, despite the American victory at Yorktown: the capital found itself gripped by a sense, probably exaggerated, that it was experiencing a crime wave, by deep-seated fear of a slave revolt in the aftermath of the flight of many blacks to the British, and by uneasiness over the imprisonment and trial of numbers of loyalists who had been brought to Richmond from throughout the state. Yet, in the end the populace remained essentially committed to the American cause; the state government was successfully established in Richmond, and a municipal government was chartered and organized; and July 4, 1783 arrived amid a memorable round of illuminations, fireworks, banquets, and balls.

Indeed, one strength of Ward and Greer's study, over and above its undoubted utility for those spe-

cifically interested in the early history of Richmond, is the extent to which their account reinforces a conclusion already suggested in other recent studies of the Revolution: the more closely one traces the War for Independence through its darkest days the more one finds serious dislocations in the life of communities directly touched by the war, a growing weariness among the citizenry, and yet in the eventual victory a release from such frustrations and the enshrinement of the Revolution in the popular imagination.

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GEORGE C. ROGERS, JR. and DAVID R. CHESNUTT, editors. *The Papers of Henry Laurens*. Volume 6, Aug. 1, 1768–July 31, 1769. Columbia: University of South Carolina Press, for the South Carolina Historical Society. 1978. Pp. xxv, 660. \$27.50.

From mid-1768 to mid-1769 Henry Laurens, a wealthy and conservative South Carolina merchant, was drawn ever more deeply into the American colonists' protest against British colonial policy. By taste and temperament Laurens had been inclined to be "a Parliament man," as he put it, upholding the royal prerogative and Parliament's American policy. He had disliked the Stamp Act, but even more he had feared the consequences of an overzealous reaction to it, and thus he had opposed even the comparatively mild response of naming delegates to represent South Carolina in the Stamp Act Congress. But by 1768 he had personally felt the lash of the colonial administration. Three of his vessels were seized under the laws of navigation on flimsy pretenses, and, although he was cleared of wrongdoing, he was assessed a large part of the court costs, including the fee of the vice-admiralty judge, fees which in one case exceeded the value of the vessel.

A large proportion of the papers published in this volume bear upon Laurens's quarrel with the customs officers and searchers and with Egerton Leigh, the vice-admiralty judge. If it is true, as many historians believe, that the estrangement of the American colonies from the royal government during the 1760s resulted not so much from Great Britain's colonial policy itself as from the implementation of that policy by greedy and corrupt placemen in America, these papers furnish strong support for the argument. Somehow the historian has to account for the fact that in many cases well-to-do and conservative Americans, who would seem to have had a large stake in the maintenance of the legitimate government, gave strong support

to the rebellion against British authority. The experience of Laurens as revealed in this volume throws much light on the question. The papers show how a scrupulous and well-intentioned man could be heavily penalized because of the rapacity of "customs racketeers." The two longest and most interesting documents are Laurens's harsh indictment of Judge Leigh, full of invective and sarcasm, which was printed in several colonial newspapers, and Leigh's even more intemperate reply, "The Man Unmasked." Leigh was Laurens's nephew and former close friend, but their quarrel was bitter and personal. Laurens directed most of his fire at the misbehavior of the judge and customs officials, but he also raised some sharp questions about the legality and constitutionality of such practices as extending vice-admiralty jurisdiction, which was originally limited to the high seas, to other situations. The practice was discriminatory because it was used only against the Americans, who were thereby deprived of their constitutional and common law protection.

Most of the papers in this volume pertain to business and legal affairs. The editors have been selective and estimate that somewhat more than half of the papers will be printed, most of the remainder being too technical and tedious to interest more than a small number of specialists. The excellence of the editorial work in this volume, as in the first five, gives us confidence that the job of selection has been the product of good judgment.

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JAMES THOMAS FLEXNER. *The Young Hamilton: A Biography*. Boston: Little, Brown. 1978. Pp. xiv, 497. \$15.00.

Thomas Jefferson, who was prepared to believe the worst about Alexander Hamilton, would have approved of this book. Hamilton's mind, claims James T. Flexner, "never matured." He was the "most psychologically troubled of the founding fathers." Part paragon, he was also "a semi-madman who sought from the world an ever-denied release from inner wounds." Thus, promises Flexner in a provocative introduction, "we shall find him behaving upon many occasions in ways so extraordinary that they are hard to believe." The reader immediately wonders what Hamilton did to justify Flexner's thesis. The literature, both pro and con, on Hamilton concurs that he was personally aggressive and egocentric, that he yearned for military glory, that he sought power, that he was a supreme nationalist, that he was not overly scrupulous as to means so long as the ends were justified. Such a description would fit a good many

prominent figures of the revolutionary and early national periods. Obviously, Flexner means something more—that Hamilton was a sick and tortured individual whose irrational actions can only be understood by their grounding in his West Indian youth.

By combining fact, speculation, and supposition, Flexner reconstructs in the first five chapters a believable account of Hamilton's sorry upbringing: his illegitimate birth, his father's bankruptcy, his mother's promiscuity, his economic destitution. Hamilton was indeed scarred, and many of his later attitudes—his cynicism, his fear of emotional attachment, his fierce need to achieve, his low regard for mankind, his contempt for money grubbers yet respect for the power of wealth—can be traced to these years. But one can, and Flexner does, ascribe too much to Hamilton's youthful experiences. He has conjured up a psychologically disturbed figure and then views much of Hamilton's subsequent behavior as "irrational" or "hysterical" or "dangerous" or "outrageous." In virtually all cases this reviewer remains not only unconvinced but also appalled at Flexner's interpretations.

Thus, when Hamilton penned a light, bantering note to a young lady, hoping to open a correspondence, Flexner seizes upon an innocuous remark about matrimony as indicative of Hamilton's "determination to come clear in a quite irrational nakedness." When Hamilton wrote in anger, demanding to know the identity of a person spreading rumors about him, Flexner says his "protestations . . . struck a hysterical note." But such demands usually do. It is typical of the genre. When Hamilton asked Washington for an independent military command, Flexner believes Hamilton was hallucinating and labels his request "one of those letters, characteristic of Hamilton, which seem rational on the surface, but if dug into reveal ideas far from rational." This reviewer finds the letter unexceptional—that of a young aide who by his own admission is tired of desk work and yearns for combat. When Hamilton explained the facts of his break with Washington in February 1781 to his father-in-law, he added that "a public knowledge of the breach would, in many ways, have an ill effect. It will probably be the policy of both sides to conceal it, and cover the separation with some plausible pretext." To Flexner no episode better illustrates "the megalomania that haunted the uncontrolled part of Hamilton's mind." Did a relatively unknown aide, asks Flexner, "really believe" that his "personal disapproval" of Washington would "seriously" interfere with the war effort? Hamilton never made that claim. His words really are nothing more than an example of understandable self-puffery. This re-

viewer can think of dozens of examples of younger men overestimating their own importance in comparable situations without being called megalomaniacs.

After tracing Hamilton's career to 1783, Flexner concludes the book with a brief final chapter covering the last twenty years of his life. The theme is that, while Hamilton's "genius built national unity, his psychic wounds sowed disunion." Many Americans must have been suffering from a similar trauma.

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HAROLD C. SYRETT, editor. *The Papers of Alexander Hamilton*. Volume 25, *July 1800–April 1802*. New York: Columbia University Press. 1977. Pp. xiv, 646. \$20.00.

The period covered in this volume was the nadir of Hamilton's life, both political and personal: the windup of frustrating military duties; the end of his influence with the holdover Adams Cabinet; the debacle of the 1800 election; his resultant loss of party leadership and growing political and party isolation; and by the death by dueling of his eldest son, Philip. One of the few things to give him solace was building his country house, "The Grange."

Perhaps the most interesting document printed is Hamilton's letter (August 26, 1800), addressed to William Jackson but sent instead to James McHenry, on the "real history" of his birth. The goad, the editors imply, was a derogatory newspaper comment. A contributory impetus may have been his recollection of a recent remark about his birth by President Adams, as relayed by McHenry. To my knowledge, the letter—whose source is "Copy, Francis Baylies Papers, Library of Congress"—has never before been published, at least not identified as Hamilton's. There is apparently no copy in McHenry's papers or draft in Hamilton's, but its authenticity is unquestioned because of the specificity of McHenry's reply. Hamilton's statements therein about his maternal grandfather, paternal grandparents, and father accord with findings of Atherton, Ramsing, Larson, and Mitchell. Some credence ought, therefore, to extend to Hamilton's recounting, thirty-two or more years later, of what he knew, believed, and had been told by or about his mother. Although he asserts concerning who his parents were that he had "better pretensions than most . . . who in this Country plume themselves on Ancestry," he indirectly acknowledges his illegitimacy.

John Church Hamilton must have had access to the letter because in his *Life* of his father he quotes (without noting the source) up to where Hamilton says "the second marriage was not lawful." Writing while Hamilton's widow still lived and without the frankness of the eighteenth century, the son glossed over what Hamilton called the "stain." Conceivably, widow or son destroyed the draft, although Allan McLane Hamilton mentions the letter in his biography of his grandfather. Thereafter, it drops from sight. Subsequent biographers cite Hamilton's son for the varying amounts of information they choose to believe. His latest biographer, acknowledging use of the Syrett edition, apparently has not seen the letter and refuses to believe anything the filiopietistic son wrote on the subject. The editors are to be congratulated on their discovery, but they might have emphasized the importance of the letter to Hamilton biographers and its bearing on his family's treatment of what he may have told them.

Syrett and the editors continue their excellence in quality of annotation and quantity, usually filling in the cast of characters to the right amount. They sometimes fail to support Hamilton's points (Address to the New York Electors) or merely adduce opponents' arguments (Adams letter) or cite only opposition newspapers, until the founding of the *New-York Evening Post* by Hamilton and others in November 1801. A few quibbles: by 1800 Georgetown was not in Maryland; and the citation of the letterbook of the Commissioners for the District of Columbia is slightly confused. For those who will not read the volume chronologically, the editors might have tempered their criticism of "The Examination" (of Jefferson's first annual congressional message) by noting that the first installment was published only three weeks after Hamilton buried his son. Early volumes of the *Papers* were more generous with illustrations: why are there none of Philip Hamilton or of The Grange?

As this is the first of the Founding Fathers documentary projects to finish (thanks, as they say, to Aaron Burr), will the forthcoming final volume—dare we hope for two?—end with Hamilton's last letter, or will the editors find a way to complete his history by including documents about settling his estate and providing for his family?

MARYJANE DOWD
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ROBERT M. JOHNSTONE, JR. *Jefferson and the Presidency: Leadership in the Young Republic*. Ithaca, N.Y.: Cornell University Press. 1978. Pp. 332. \$15.00.

Robert M. Johnstone, Jr.'s purpose has been to study Jefferson's presidential leadership using the

theories developed by social scientists to explain modern political leadership. In particular, Johnstone employs Richard Neustadt's "bargaining model" and uses role analysis, relying on the key variables of role, style, and political setting. If employing these conceptual models as a means of ordering what already is known about Jefferson as president is sufficient justification for the publication of this book, then it succeeds. But, if we expect the author to contribute to our understanding of Jefferson's presidency by offering the results of new or expanded research in the sources, then it is a disappointment.

There can be little quarrel with the basic conclusions of the work. "It is a central thesis of this book," Johnstone writes, "that Jefferson's presidency marked the pioneering effort in erecting a working model of presidential leadership characterized by persuasion and the cultivation of influence. Jefferson was the first president willing to implement the bargaining relationships that could enhance presidential influence, and he did so with great natural skill and patience. . . . Through his use of . . . the political party, Jefferson was able to span the chasm that separated the elected institutions of government from one another" (p. 14). Johnstone also emphasizes that "one of Jefferson's great strengths as president . . . was the fact that he embraced in his own ideals and assumptions of political reality so many of the central values of the larger political community" (p. 18).

In developing these themes, Johnstone discusses Jefferson's relations with the Cabinet, Congress, the judiciary, and the Republican Party. He repeats the interpretation of Leonard White that Jefferson was not interested in day-to-day administration and accepts the widely held view that his administration was a triumvirate of Jefferson, Madison, and Gallatin. He does not support James S. Young's denial of the existence of parties in Congress nor Young's view that Congress was "at a distance and out of sight." His discussion of the struggle with the judiciary consists mainly of a review of familiar themes (the repeal of the Judiciary Act of 1801, *Marbury v. Madison*, the impeachments of Pickering and Chase, and the Burr conspiracy) without offering either new evidence or new insights. The treatment of parties is superficial and at times inaccurate, as, for example, in the statement that in Virginia "there was little incentive for elaborate party machinery, and party management was accomplished through informal arrangements among elites" (p. 220). But the Republican party in Virginia had a formal, functioning structure of state and county committees. The author devotes a chapter to Jefferson and the embargo as a case study of the limits of presidential leadership, but he never moves beyond a review of

the issue to deal in depth with presidential decisions and leadership.

The book is unevenly documented and sometimes lacking at crucial points. For example, there are no footnotes on page 140, which treats such important topics as Jefferson's drafting of bills, department heads testifying before congressional committees, and the secretary of the treasury attending party caucuses. All of these are matters that deserve elaboration and require documentation. The book shows that present-day leadership theories can be employed to examine early presidencies; but, unless they are accompanied by a broadening of our knowledge of the past, such exercises have limited value to historians.

NOBLE E. CUNNINGHAM, JR.
University of Missouri, Columbia

ROBERT A. RUTLAND and CHARLES F. HOBSON, editors. *The Papers of James Madison*. Volume 11, 7 March 1788-1 March 1789. Charlottesville: University Press of Virginia, 1977. Pp. xvi, 471. \$15.00.

This is the first volume of *The Papers of James Madison* to be published by the University of Virginia Press. Aside from the title page and the statement of that fact in the preface, there is no discernible break between this and the first ten volumes. Volume eleven continues the high standards of editing and book printing that have always characterized this series.

Volume eleven is, nonetheless, exceptional. It chronicles a vitally important year in American history. Between March 7, 1788 and March 1, 1789, the Virginia state convention ratified the Constitution, the Confederation Congress passed the first federal election resolution, and Virginia elected federal representatives and senators. The documents in this volume richly illuminate all three of these events and, of course, Madison's part in them. Therein lies the difficulty. Because the focal point of this volume is James Madison, the documents, be they Madison's papers or excerpts from the journals of the state convention or Confederation Congress, magnify Madison's role. This is not to say that Madison was not an important figure in New York or in Richmond, but these documents can leave a false image of his importance. At the Virginia Convention, for example, the reporter frequently could not hear Madison because he spoke so softly. In New York following ratification, the major stumbling block to passage of an election resolution was the future location of the federal capital. In that debate Madison remained a firm proponent of the banks of the Potomac until the very end.

Madison was, of course, a major figure in the

first federal election in Virginia's fifth district. And, to the extent that Madison's election contributed to the eventual adoption of a bill of rights, it is of major importance. But the election itself was not unlike similarly contested elections in New York, North Carolina, and Georgia. My point is that this volume and its companions, because of the breadth of their documentation and the excellence of the editing, will encourage us to assign to Madison a greater role than perhaps is appropriate. The volumes also encourage us to accept Madison's perceptions and point of view. Yet at times Madison was misinformed—for example, about the intentions of the Pennsylvania Antifederalists in the first federal elections. He also differed with other Federalists about the meaning of the Constitution and the intent of the framers—notably with Hamilton regarding the nature and power of the federal courts.

To the extent that this volume encourages us to accept Madison's views uncritically, it serves ironically to retard rather than expand our understanding of this crucial period of America's past.

STEVEN R. BOYD
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CARL E. PRINCE. *The Federalists and the Origins of the U.S. Civil Service*. New York: New York University Press, 1977. Pp. xiii, 381, \$17.50.

This book undermines several previously established ideas about American politics from 1789 through 1800. Far from avoiding partisanship, George Washington instructed his chief subordinates to fill the civil lists with reliable supporters of his federal system. Such leading administrators as Alexander Hamilton and Timothy Pickering, though honest and efficient themselves, repeatedly tolerated inefficient and even dishonest men who had useful political connections. Every sort of appointee, including attorneys, clerks, and judges, acted to advance party interests or to defend the controversial measures of the national government. John Adams had fewer appointments to make than Washington but was even more partisan in making them. He also assumed from the very beginning of his presidency that politicians very close to Hamilton should be treated as political enemies.

Carl E. Prince further demonstrates that the government filled minor as well as major posts with dedicated supporters. As the civil service became the center of party organization, it also extended that organization through all of the politically active classes of society. The Federalists were never simply a collection of self-consciously upper-

class politicians attempting to keep humble folk out of active politics. On the contrary, they welcomed and rewarded men of small estates. Appointments as postmasters, for instance, often provided the margin of success to rustic merchants, printers, and tavern-keepers. All of these thought-provoking conclusions emerge from a survey of the civil service, branch by branch, region by region, and officer by officer. Prince especially scrutinizes the wealth and social connections of his office-holders and creates a sort of collective profile of them. This is mercifully free of jargon and is nicely sharpened by a group of statistical tables.

A number of historians are at present exploring the survival and extension of Old Whig political concepts in the early national period. More particularly, they are treating the Jeffersonians and Old Republicans to the kind of scrutiny that Bernard Bailyn applied to Revolutionary propagandists. So far the correspondence between Jeffersonian oppositionist ideas and actual Federalist practice has remained unclear. But if Prince is correct—and his calmly reasoned, enormously detailed narrative is most persuasive—the Federalists were indeed engaged in building a court party, complete with favoritism, nepotism, and corruption. Could there have been a government on any other basis? Were the succeeding governments of Jefferson and Madison significantly different?

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GERALD LEE GUTEK. *Joseph Neef: The Americanization of Pestalozzianism*. University: University of Alabama Press. 1978. Pp. viii, 159. \$10.75.

Most survey histories of American education credit Joseph Neef as the first Pestalozzian educator in the United States and the first in this country to author pedagogical texts in the English language. It seems reasonable, however, that historians would find more of interest in Neef's experiences than in his accomplishments. His life and thought provide remarkable illustrations of the relationships between social and educational reforms as well as of the transmission of European ideas and ideals to America during the early national period. As a deeply committed advocate of the principles of the French Revolution, Neef was led from service in Napoleon's army to work and study under Johann Pestalozzi. Neef was converted not just to Pestalozzi's humane and scientifically oriented approaches to teaching and curriculum but equally to his faith in the power of education to provide the foundation for a more just society. One cannot fully understand the basis of

Neef's long association with William Maclure, his decision to emigrate to the United States, or his participation in Robert Owen's New Harmony experiment without appreciating his commitment to the social as well as the pedagogical elements of Pestalozzian thought.

Gutek has failed, unfortunately, to avail himself of the opportunities that Neef, his associates, and his time provide. Between the opening pages, in which the author repeatedly proclaims that Neef initiated Pestalozzian teaching in the United States, and the close of the book, in which he informs us that "Neef's attempt to popularize Pestalozzianism had little impact on the course of American intellectual development" (p. 133), there is a chronicle of Neef's life (part one) and a description of his educational theories and practices (part two). The reader is denied fully drawn pictures of Neef as a man of millenarian vision and purpose and of the social, political, and intellectual world in which he lived. The centrality of education in the plans of not only Maclure and Owen but also of the moderate reformers of the period is inadequately conveyed. As for Pestalozzianism, Gutek neither acknowledges its wide appeal among American school leaders nor fully analyzes its impact upon educational thought and practice during the early years of the common school movement. To imply, as Gutek does, that Pestalozzianism virtually disappeared between the demise of the New Harmony experiment in 1828 and the appearance of Henry Barnard's *American Journal of Education* in 1836 is to ignore enthusiastic endorsements of it in the workingmen's press between 1828 and 1836 and subsequently in the writings and speeches of Horace Mann.

Gutek's book does provide a good deal of useful information for the student of educational history, but we must look to the future for a definitive work on Joseph Neef and the Americanization of Pestalozzianism.

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DIANE LINDSTROM. *Economic Development in the Philadelphia Region, 1810-1850*. New York: Columbia University Press. 1978. Pp. viii, 255. \$16.50.

Historians of American economic development have generally phrased their analyses of the antebellum period in terms that would warm the heart of Henry Clay: the revolution in internal transportation after 1815 brought forth a national market, in which the three great sections exchanged their increasingly specialized commodities (manufactured goods from the North, cotton from the

South, and grain and meat from the West) to the advantage of all. Sectional specialization and intersectional transportation and trade, in other words, were the basic mechanisms of national economic growth. In recent years a few dissenters, most notably Albert Fishlow, have pointed out that the sections were far more self-sufficient than this national market theory would allow and that until at least the 1840s intersectional trade was only a minor component of the economy as a whole. Yet, even as early as 1840, it was clear that critical structural changes and some real growth had taken place. How, then, do we account for this early stage of economic development?

Diane Lindstrom supplies a well-argued and convincing answer to this significant question by analyzing in great detail the economic development of Philadelphia and the forty-five counties that constituted that city's urban region. Before 1840 demand originating *within the region itself* gave Philadelphia's fledgling industrial sector its major boost and caused its "structural transformation": "The size, timing, and rate of growth of Philadelphia-hinterland commerce highlight the significance of the intraregional market for Philadelphia's structural transformation. . . . The hinterland population purchased from the core some 20 to 25 and possibly 30 million dollars more in the late 1830s than in the late 1810s. This sum, even if one-half was spent upon foreign wares, would be sufficient to explain the transformation at Philadelphia (p. 112). Of course, the new hinterland market was itself a product of development in the forty-five counties, and in sketching this development Lindstrom emphasizes the very factors that figure so largely in the histories she is trying to supplant—new, cheaper transportation facilities, areal specialization, and intensified market exchange between specialized areas. The difference is that the transportation revolution is presented here as a considerably more localized affair, an event that built regions before it built a nation. And, more importantly, in the construction of the "fully integrated and essentially self-sufficient regional economy" (p. 122)—in which manufactured goods, capital, food, coal, and iron flowed across distances not exceeding two hundred miles—we find the explanation for the first phase of accelerated economic development.

Moreover, there is a second phase that stops well short of national integration. After 1840, Philadelphia's rapidly growing industrial sector reached increasingly beyond the limits of the city's hinterland. But as it did so, it reached not to the distant South and West but to the other cities of the East. The hinterland itself, led by its anthracite mines, found the same Eastern markets. These were the years of very rapid growth, to which the

preceding decades of growing intraregional trade had been pointing, and Lindstrom is as forthright as possible in claiming that the "boom in the Philadelphia region's economy came primarily from specialization for Eastern markets" (p. 184). Thus, her entire analysis confirms what she has called "the Eastern demand model," according to which Philadelphia's "structural transformation" and its whole region's economic growth occurred first in the context of trade within the region and then in the context of trade within the section. Only later, at some unspecified time beyond the chronological bounds of this book, would national markets play a significant role in shaping the Philadelphia region's economic development.

Lindstrom does not merely propose her "Eastern demand model." The bulk of the book consists of a detailed and careful analysis of regional, sectional, intersectional, and international trade statistics and commodity flows, production and income data, and even some of the things that the region's entrepreneurs had to say about their own affairs. She appears to be in full command of her data and uses them boldly—at times too boldly, perhaps, given the acknowledged unreliability of many of the quantitative records of this statistical Dark Age. But the case is well made and is ultimately convincing. The major questions that arise concern not the argument but its implications. Was the Philadelphia region's experience typical of other Eastern regions, or do such factors as its particular geographic setting make it unique? Philadelphia enjoyed a particularly rich hinterland but suffered under relatively poor connections with the West and South—a combination that fits very nicely with the developmental pattern that Lindstrom describes. But New York's intersectional connections were much better, and Boston's hinterland was less productive. Were their developmental sequences the same as Philadelphia's nonetheless? Lindstrom discounts the geographic differences and concludes that the Eastern demand model is applicable to the other regions of the East. But the point will remain open until parallel studies of those regions are conducted. And what of the ultimate emergence of integrated national markets? When and by what processes was this stage finally reached? The value of this book lies precisely in the fact that, with its appearance, these remaining questions have become very urgent.

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JONATHAN GOLDSTEIN. *Philadelphia and the China Trade, 1682-1846: Commercial, Cultural, and Attitu-*

dinal Effects. University Park: Pennsylvania State University Press. 1978. Pp. 121. \$9.50.

Jonathan Goldstein's thesis is that Philadelphians engaged in the China trade and their intellectual friends esteemed Chinese civilization and projected this favorable image to the press. His sample consists of ninety-five unidentified Americans (p. 7). His use of the records of the few traders examined arouses suspicion that he has little hard, supportive evidence. For example, he quotes Stephen Girard specifically for opinions on ship construction, trade items, and opium but never on Chinese culture. He quotes Girard's praise for two hong merchants, but such friendly references by Americans (or Englishmen) are hardly new. To infer from them general admiration for the Chinese and their culture is highly questionable. Goldstein turns to the chinoiserie fad for additional support. Girard collected Chinese porcelain, *ergo* Girard admired the society that produced it. This is dubious.

The author's deceptively selective use of documents, omission of contradictory evidence, and misrepresentation of published works have created some grave distortions. For example, he quotes Thomas Randall to demonstrate an American "policy of passivity" when faced with official abuse (p. 61). But in this document, Randall described an armed boat of his joining others in an aggressive sortie "to frighten the Chinese" and expressed disgust over a final compromise "for in point of real force or consequence, the Chinese are considered by most persons who have seen them, as contemptible, however importantly they think of themselves." Randall also recommended "active protection" for Americans, citing the Royal Navy as a model (Alexander Hamilton, *Industrial and Commercial Correspondence* [1928], 140-41). Goldstein writes that the *USS Congress* received "a cool if not unfriendly reception from Americans" in Canton in 1819 (p. 63). His next note cites Paullin, who wrote nothing of the sort. Finally, he informs us that traders petitioned Congress in 1839 to avoid war in China, but not that months later other traders asked Washington "to act in concert" with Britain and send a "naval force" immediately (House Document 40, 26th Congress, First Session).

Goldstein charges me with having accused the traders of "racism" (p. 5, *passim*). In fact, I wrote: "the assumption that American traders were free of racial prejudices before 1840 is well supported by these records" (*Unwelcome Immigrant* [1969], p. 21). He claims that Du Ponceau's "major scholarly contribution . . . was his observation of the ready comprehensibility of the written Chinese character among c'iverse Asiatic cultures" (p. 79). Du Pon-

ceau's thesis was exactly the opposite (*Dissertation on . . . the Chinese System of Writing* [1838], pp. xxxi-xxxii, 14, 86-87, 96ff).

Goldstein concludes by naming fifteen sinophiles, including Randall (based on the single document discussed earlier). His repeated assertion that "a positive media image of China" had "emerged in the media" (pp. 31, 33) remains unsubstantiated.

This book is poorly reasoned, inadequately researched, and replete with serious errors.

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ROBERT V. REMINI. *Andrew Jackson and the Course of American Empire, 1767-1821*. New York: Harper and Row. 1977. Pp. xix, 502. \$15.00.

Here is the first half of the Jackson biography for our time. Robert V. Remini's research has been exhaustive; his judgments are considered; his narrative is clear and absorbing, enlivened by colorful incident and skillfully woven quotation. In short, he combines the scholarly thoroughness and care of John Spencer Bassett with the narrative skill and fascination with personality of Marquis James, while adding to the merits of both the fruits of the rich Jacksonian scholarship of recent years.

What kind of Jackson is revealed through these gifts? Remini gingerly approaches the highly speculative psychoanalytic interpretation of Michael Rogin, but his caution about the early psychic history of a figure who can be known only through sparse and largely hearsay sources leaves him short even of the persuasive psychological interpretation achieved by James C. Curtis. Nevertheless, Remini pictures a Jackson scarred by his mother's wartime "desertion" and his early orphaning. Somehow, out of these early experiences, came Jackson's youthful wildness, his touchiness about his honor, his imperiousness, and his paternalism toward Indians, blacks, women, and perhaps (anticipating the second volume) the American people.

The interpretive merits of this volume are many. More clearly than any other biographer, Remini show that Jackson did play fast and loose with the rules of social propriety in his adulterous "marriage" to Rachel Donelson. He unravels surely the tangled skein of early Tennessee politics in which Jackson rose so quickly to prominence. He delineates with clarity and balance Jackson's early and chastening experiences in merchandising and land speculation.

Remini's pre-eminent interpretive contribution is the understanding he gives us of Jackson's absolutely central role in expansionist diplomacy and

Indian policy in the early Southwest. Research in the Spanish archives revealed the Spaniards' growing consciousness of Jackson as the principal threat to their position on the American border. For the Tennessee general early became convinced that the United States must acquire not only the Floridas but also Cuba, Texas, and eventually all of Spain's North American possessions.

It was Jackson's almost superhuman determination in the Creek War that opened the possibility; it was Jackson's brilliant defense of New Orleans that kept the possibility open (Remini persuasively revives the argument that defeat at New Orleans might have lost the whole Southwest); and it was Jackson's high-handedness in the First Seminole War that pushed the cautious Monroe administration into supporting John Quincy Adams's transcontinental diplomacy resulting in the treaty of 1819-21. In all of these episodes Jackson's military operations are described with greater clarity than any previous author has achieved.

At the same time it was Jackson who forced upon the federal government a ruthless policy of driving native Americans from the Southwest to make way for white settlers. This was not just a matter of his bloody crushing of the Indians' ability and will to resist. In addition, in person or through deputies, he negotiated the series of "treaties" whereby native Americans surrendered nearly all of their southwestern territories under the none-too-subtle threat of Jacksonian retribution, supplemented by judicious bribery of the chiefs. In these treaties Jackson made Indian removal the ultimate objective of federal policy.

My only caveat is the degree to which the anti-Indian bias of Remini's white sources has tinted his prose. Native Americans are repeatedly referred to as "savages," albeit within quotation marks. They "butchered" settlers, perpetrated a "senseless massacre" at Fort Mims, and desisted only when "satiated by their savagery" (pp. 46, 190). On the other hand, says Remini, white atrocities must be viewed "in the context of the nineteenth century," when "a powerful need existed throughout the country . . . to subdue the Indians and expel them from territory that was believed to be essential to national expansion and the defense of the country" (p. 340). However "disgraceful" some aspects of Jackson's Chickasaw treaty, "still they could not diminish the magnitude of the stupendous cession of land" (p. 340).

All of this makes it easier to understand how Jackson emerged as such a formidable contender for the presidency just beyond the termination of the present volume. Remini's previous work has demonstrated his mastery of the technical inner history of domestic politics. Here he has demonstrated a broadened command of the geopolitical,

military, and diplomatic factors involved in American expansion. If his succeeding volume demonstrates a comparable mastery of the broad social and economic forces reflected in the politics that Jackson was to dominate, this will indeed be our Jackson biography for a long time to come.

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DOUGLAS C. STANGE. *Patterns of Antislavery among American Unitarians, 1831-1860*. Cranbury, N.J.: Fairleigh Dickinson University Press. 1977. Pp. 308. \$15.50.

Douglas C. Stange's *Patterns of Antislavery among American Unitarians, 1831-1860*, treats an interesting problem. Although a fair number of important abolitionists were Unitarians, it seems as if there should have been more of them than there were. The sect, unlike most others, had no Southern branch of any consequence, and it was concentrated in the antislavery strongholds of the Northeast. Yet it was sluggish about taking a stand against slavery, much to the dismay both of reformers and (by the mid-1840s) of European Unitarians. Stange seldom attempts to explain in depth the denomination's behavior or even to follow up on his own occasional suggestions about the relationship between abolitionism and Unitarian theology, class composition, and organizational structure. Instead, he spends most of his time deploring the indifference of the majority and describing, with ample praise, the tribulations of the antislavery minority.

Stange sorts Unitarian abolitionists, and all others, into three "patterns"—antislavery as "religion," "philosophy," and "politics." Initially, according to him, these were relatively distinct positions, but they converged in the 1850s. He weakens the argument, however, by devoting only a tenth of the text to events after 1850. In any case, the utility of these categories is open to question. Religion comprehends Garrisonian radicals; philosophy's exemplar is William Ellery Channing; and the last pattern includes everyone "who saw in power in politics the way to destroy slavery" (p. 36). This manner of dividing abolitionists often obscures more than it reveals, particularly when applied to important individuals like Lydia Maria Child or (to use a non-Unitarian example) Lewis Tappan. Such men and women were too extreme for philosophy, as Stange defines it, not terribly committed to politics, and often out of step with Garrison, even though every bit as religiously inspired as he was. It is hard to see how Stange's groupings of abolitionists could be any more re-

vealing than the ones commonly in use among historians.

It is not that Stange lacked space for more rigorous analysis. His concern for biographical details and his sometimes overly vivid prose take attention away from the central question of the ties between Unitarianism and antislavery. For instance, he spends the best part of a paragraph in a needless defense of Maria Weston Chapman's beauty, a matter better treated in a clause. In another paragraph he spins out an elaborate image of Channing as a conductor refusing abolitionism (the "drums and brass section") a "solo performance" in the "orchestra of morality" (p. 76). The imagination behind such a passage would have been better directed toward a close examination of Unitarian denominational structure, a significant issue not confronted until long past the mid-point of the book.

Such weaknesses in conception and execution diminish the value of Stange's work. The book does present useful information about particular abolitionists, but there is room for still other interpretations of Unitarianism and antislavery.

RONALD G. WALTERS
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J. MILLS THORNTON III. *Politics and Power in a Slave Society: Alabama, 1800-1860*. Baton Rouge: Louisiana State University Press. 1978. Pp. xxiv, 492. \$22.50.

J. Mills Thornton III has written a provocative book but a book that happily is not quite as provocative as the book jacket suggests. The blurb informs us that "Thornton argues that slavery . . . coexisted with and supported the democratic and egalitarian faith." Thornton argues, rather, that white Alabamians felt something like this. No doubt it is his failure to moralize about these sentiments that misled one of his editors. Actually, one of the strengths of the book is its author's empathy with antebellum white Alabamians, which enhances his attempt "to place upon the reader the spectacles through which antebellum Alabamians peered out at their frightening world" (p. xvii). Some readers may be disconcerted at Thornton's failure to insert the adjective "white" before the phrase "antebellum Alabamians," for certainly it belongs there.

In addition to being provocative, Thornton's book—which is a prize-winning adaptation of a doctoral dissertation of uncertain age—is opinionated, argumentative, overwritten, at times pretentious, learned, richly researched, and forcibly—if not always persuasively—argued. It is also full of nuance and any number of other good and not so good things. Very much like the fabled little girl, it

displays most dissimilar traits, sometimes in its treatment of a single theme.

Although the subtitle might suggest roughly equal treatment of a sixty-year period, the book devotes about two-thirds of its space to the 1850s and almost all of the rest to the Jacksonian period. In view of the crucial and dramatic nature of that fateful decade, one can hardly fault Thornton for focusing on the 1850s. Thornton regards few previous accounts of the sectional crisis as satisfactory, impartially flaying neophytes and master historians alike for their wrongheadedness and their failure to interpret their data as he would have done. It is puzzling that he has failed to read David M. Potter's marvelous last book on that crisis, published more than two years ago. One wonders just when Thornton completed his study, in view of the almost total absence from his bibliography of publications of the 1970s and his reference to a book that came out in 1974 as "a work published after the completion of my present study" (p. 338).

There are numerous flaws, in some cases glaring weaknesses, in Thornton's book, and by my reading its deficiencies clearly outnumber its good points. It is too full of unnecessarily long, turgid quotations from speeches, letters, and editorials, and it often generalizes about the alleged feelings of a group on the basis of no more than one or two such statements. Untranslated Latin terms are worn a mite too proudly on the author's sleeve, the text is sprinkled with "diachronically," "dispositive," "asseverations," and other terms of the sort that give scholarly discourse a reputation for preciosity. An alert editor might have eliminated Thornton's statement that "as the eunuch shapes the desires of the emperor, as the motion picture mogul gives direction to the tastes of the masses, to that extent, at least, politicians led the people of Alabama" (p. 162).

The book is at times deterministic with a vengeance, as, for example, in the statement that "the events of 1850 represent the natural consequence of the political order which we have described." It can be rigidly uncompromising in interpreting an event, such as the vote won by Zachary Taylor in Alabama or a mood or an idea, treating as though definitive the kind of judgment that a historian ought to know is arguable. Thornton tells us what Alabamians thought "without question" (p. 221). He at times strains both his evidence and his language to justify tenuous interpretations. To show that in the 1850s events of unprecedented character transformed the mood of the people, Thornton, after artfully summarizing these events, writes: "dazed, indecisive, frightened, Alabama's masses stumbled towards the final crisis" (p. 306). Ludites roaming the hills and white Alabamians divorcing and murdering one another at higher rates than ever are invoked in a dubious exercise in

social psychology to bolster an unconvincing argument. And one yearns not for moralizing but for historical analysis of the judgment Thornton offers, that for Southerners, the "ideals of democracy and equality . . . were bound up with [the defense of] slavery" (p. 402).

The catalogue of things negative could be extended. And yet the value of Thornton's book is not truly communicated by a listing of its hits and misses. I do not know whether its strengths outweigh its weaknesses. What I do know is that, despite all its problems and the inability of its heavy descriptions and argument to induce any exhilaration in the reader, the book is valuable and weighty, in a positive sense of the latter term. I know of few state histories that convey so well the complexity and the paradoxical element in political affairs.

Thornton's discussion of Whig and Jacksonian parties is informed and insightful, effectively relating realistic and idealistic elements in their performance. Thornton modishly reports socioeconomic evidence on political leaders, but he is wise enough to know that their public actions are more important than, and not fully explained by, such data. Alabama politics are dealt with masterfully. Thornton describes and explains how things worked at all levels of Alabama politics, and his interpretations are at times not only provocative but illuminating and even wise.

The best thing in the book is its detailed examination of the movement toward secession and of the role of the fire-eaters, particularly William Lowndes Yancey, in that movement. One need not agree with Thornton's views on these matters to find them stimulating and worthy of respect. The great strength of Thornton's mental world is that, as in life, nothing in it is simple. Fire-eaters may have lived in an intellectual never-never land, peopled with fanatical "abolitionists" like Abraham Lincoln. And yet, as Thornton demonstrates, they were themselves ideologically heterogeneous, at least in some respects, and as capable of pragmatic manipulation and even compromise—of sorts—as the major party politicians they sought to overthrow.

The Louisiana State University Press is to be complimented for producing an attractive book that has footnotes at the foot of the page, where they belong.

EDWARD PESSEN
Baruch College,
City University of New York

H. CRAIG MINER and WILLIAM E. UNRAU. *The End of Indian Kansas: A Study of Cultural Revolution, 1854-1871*. Lawrence: The Regents Press of Kansas. 1978. Pp. xiii, 179. \$12.50.

The authors of this book state that their objective is "the breaking of new ground by dealing with subjects and sources largely untreated in the published literature on the American Indian." Since their subject is the removal of Indians from Kansas, facets of which have attracted other historians, and since they principally use sources other historians have employed, perhaps they are claiming too much. Nevertheless, what they have done is sufficient cause for self-congratulation, because they have delved more deeply into the subject than any previous writers. That the removal of the Indians from Kansas was a sordid episode in American history is generally known. What H. Craig Miner and William E. Unrau do is document in greater detail than ever before how in a quarter century the number of Indians in the state declined from over ten thousand to less than one thousand, while millions of acres of Indian land passed into the hands of whites. There is the usual cast of negligent officials, rapacious settlers, venal interpreters, and double-dealing mixed bloods.

Using case histories of treaty negotiations and other forms of land disposal, the authors depict the process by which the tribes were persuaded to abandon their Kansas holdings and agreed to move to what would become Oklahoma. As they make clear, the Indians were not actually driven from the land, but they were subjected to what the authors call carrot-and-stick tactics. The Indians were alternately harrassed and tempted until their resistance to removal was broken. What made this relocation particularly tragic was that most of the tribes involved already had been moved at least once and had been promised that Kansas was to be their permanent home.

The authors show that Indian Service personnel frequently failed to safeguard the Indian interests entrusted to them. Even the wife of one commissioner of Indian affairs shared in the profiteering. A governor of Kansas and sundry military personnel, to whom the Indians had to look for protection, also devoted time and energy to land speculation at the expense of Indians sorely in need of support. It is little wonder that by the time the New York Indians finally left their Kansas reserve, they were outnumbered on it by white squatters 2,202 to 50.

Nor do the authors spare the Indian leadership, which they find to have been incompetent and faction ridden, if not corrupt. Their chapter entitled "The Government Chief" is particularly effective, demonstrating how the government selected and maintained the type of tribal leader who would be amenable to its policies.

The role of business enterprise in eroding Indian claims in Kansas is largely the story of railroads. It is a sad commentary on Indian-white relations that the authors conclude that the tribes probably

lost less in dealing with these grasping entrepreneurs than in disposing of their land to the government or private individuals.

The authors' style is sometimes rather prolix and unnecessarily convoluted. An example of them at their worst is this sentence: "To aggravate the situation, oversimplified beliefs in the virtues of evangelical Christianity in its assumed relationship to democratic fulfillment were frustrated by the more earthly behavior of charlatans deficient in moral performance." Fortunately, these stylistic lapses do not occur often enough to seriously flaw an important book that is a textbook on the manner in which Indians lost their land.

WILLIAM T. HAGAN
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College at Fredonia

WILSON JEREMIAH MOSES. *The Golden Age of Black Nationalism, 1850-1925*. Hamden, Conn.: Archon Books. 1978. Pp. \$17.50.

The Golden Age of Black Nationalism is a book that inevitably will stir controversy. This is by design. Wilson Jeremiah Moses indicates that "the book is intended to provoke controversy and to provide a new thesis concerning the Golden Age of Black Nationalism—that during these years [1850-1925] the ideology was conservative rather than radical" (p. 11). And, although he makes it clear that his book is "not a definitive study of black nationalism and makes no such pretensions, . . . it is a challenge to those who have viewed black nationalism as if it were invariably a leftist movement."

The period under study, 1850-1925, would hardly seem to be a "golden age" for black Americans, no matter what their pan-African views. One can think of few drearier periods in black American history, and even the enthusiastic author of the blurb on *Golden Age's* book jacket concedes the period "would hardly seem to mark a golden age." Of course, the writer finds that out of the tension and anxieties of the 1850s a literary outpouring of black nationalist sentiment flooded this country, one that persisted until the rise of Marcus Garvey and the "ideological Pan-Africanism of W. E. B. DuBois." But the term "golden age" should imply something positive about a person, subject, or movement, and many will wonder whether the era was as productive or positive for the black nationalist movement as this title implies.

No doubt, too, controversy will swirl around Moses's definition of black nationalism. His definition is so broad that it becomes murky at times and frequently contradictory. Black nationalism "has many forms. . . . In Africa it has meant simply the desire to be rid of foreign domination. In

the United States it has often meant either the desire to return to Africa and establish a modern black state or one in the Americas" (pp. 19-20). At other times it is identical to the idea of pan-Africanism, "in which cases it has not necessarily been concerned with the establishment of a geographical state but only with asserting a sense of kinship among the African peoples. If there is one *essential* quality of black nationalism, however, it is the feeling on the part of black individuals that they are responsible for the welfare of other black individuals, or of black people as a collective entity, simply because of a shared racial heritage and destiny."

This reviewer does not wish to be hypercritical of Moses. He has written a series of well-researched, well-written, and most interesting essays about some important black leaders (for example, Frederick Douglass, Booker T. Washington, and W. E. B. DuBois), some obscure figures who deserve greater recognition (Alexander Crummell and Sutton Griggs), and some bourgeois black feminist groups and literary organizations. He portrays the black nationalist movement as a product of the black bourgeoisie. He views these people as essentially conservative and assimilationist blacks who believed that "Anglo-American culture" was the highest form of civilization. Moses argues forcefully that beneath the rhetorical sound and fury, DuBois's and Booker T. Washington's views were not as dissimilar as other scholars suggest. But after reading the book, one is still left with two nagging questions. Was the period 1850-1925 a golden age for black nationalism? And how does one define black nationalism in concrete, substantive terms?

SHELDON H. HARRIS
California State University
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LOUIS R. HARLAN and RAYMOND W. SMOCK, editors.
The Booker T. Washington Papers. Volume 6, 1901-2; volume 7, 1903-4. Urbana: University of Illinois Press. 1977. Pp. xxx, 661; xxviii, 574. \$17.50.

The latest two, thick volumes of the *Papers* of Booker T. Washington cover the period from January 1901 through June 1904. During these early years of Theodore Roosevelt's presidency, Washington's influence, particularly in politics, reached its peak, but his rise was also accompanied by a heightened controversy over the nature of his leadership. The works at hand provide a rich source for the study of these issues as well as other aspects of the history of black Americans in the early twentieth century.

The largest percentage of the documents are

letters to Washington, along with occasional newspaper articles and related materials. Volume six is enhanced by a number of interesting pictures, most notably the excellent photographs of Tuskegee Institute taken in 1902 by Frances Benjamin Johnston. Unfortunately, many of Washington's outgoing letters have not been preserved, a fact that is particularly evident for the early months of 1901, although their number increases thereafter. Other glimpses of Washington are provided by some of his articles, book reviews, and Sunday evening talks at Tuskegee. As in the preceding volumes, the annotation is unobtrusive but helpful. The editors have done a remarkable job of identifying innumerable obscure individuals, and in most instances they have provided a clear and concise outline of the facts needed to understand a given document.

The reader will readily find in these volumes evidence of the variety of Washington's activities as well as his unflagging industriousness. One of his key roles, of course, was that of master of Tuskegee. Although he spent much time away from Alabama, his concern for the details of Tuskegee Institute is obvious. My favorite document here is his letter of admonition to one of his teachers who committed the offense of smoking on a public street near the school. His success as a money-raiser is also illustrated with the announcement in April 1903 of Andrew Carnegie's gift of \$600,000, with its provision that a suitable amount be set aside to cover the needs of Washington and his wife. Perhaps his most interesting role during these years was that of political broker. Although Washington denied his involvement in politics, his papers reveal a steady stream of recommendations on appointments as well as activities against the lily-white Republicans. Washington may not have approved of all of Roosevelt's actions, but he was clearly convinced that support for him was in the best interests of American blacks, and he acted accordingly.

Washington's prestige, however, was inadequate to check the deterioration in the status of blacks or even to prevent attacks upon himself. These documents remind us again and again of the precarious nature of his position: witness the vehement reaction of Southern whites to his dinner with Roosevelt at the White House, the extreme timidity of such Southern white liberals as Edgar Gardner Murphy, or the controversy over the appointment of William D. Crum as collector of the port of Charleston, South Carolina. Washington's ineffectiveness in the face of determined racism is similarly illustrated by his inability to alter the disfranchisement proposals adopted by the Alabama Constitutional Convention in 1901. Ultimately, Washington accepted the idea of property qualifications for voting on the ground that these would

become incentives to industry and that blacks, once they had acquired property, would be granted the vote. He held, of course, that whites must be subject to the same requirements, but the gap between his hopes and the realities steadily widened and became a major complaint against him by his black opponents.

Some of Washington's secret activities contrasted sharply with his apparent optimism about the franchise. An example is his financial support for the case brought by Jackson Giles, a black who unsuccessfully sought to compel the voting officials in Alabama to allow him to register. Washington's most interesting secret activities were not on behalf of civil rights, however, but against his enemies. His most determined antagonist in these years was not W. E. B. DuBois, with whom Washington maintained a surface cordiality despite a growing mistrust, but William Monroe Trotter of Boston. Washington did his best to destroy Trotter's influence among blacks, and he was obviously pleased to see Trotter sentenced to thirty days in jail for his part in the so-called Boston riot in 1903.

If such activities illustrate Washington's least attractive side, they still represent only one part of an extremely busy life. Advocates of a wide variety of causes can find in the many activities of Booker T. Washington examples to fit their needs, whether favorable or otherwise. Washington will remain a fascinating figure precisely because of this diversity and ambiguity. Thanks to the first-rate efforts of Louis R. Harlan, Raymond W. Smock, and their associates, Washington is also becoming a more accessible figure. All students of American history are in their debt.

RICHARD B. SHERMAN
College of William and Mary

FRANCES RICHARDSON KELLER. *An American Crusade: The Life of Charles Waddell Chesnutt*. Provo: Brigham Young University Press. 1978. Pp. xvi, 304.

Since the lives of so many Negro authors—from Phyllis Wheatley and Paul Lawrence Dunbar to Jean Toomer and Richard Wright—were tragedies, it is comforting and pleasant to read the biography of one whose life was not. The life of Charles Waddell Chesnutt was not tragic, even though he never gained the acceptance, either as an author or as a man, that surely would have been his had he been a white man—and that he might have been had he but chosen to since he was light enough to “pass.”

Chesnutt was born in Cleveland in 1858 of free parents who had migrated from Fayetteville, North Carolina just two years earlier. And to Fayetteville they returned in 1866, because it was home. Young Charles grew to manhood there,

receiving in all but four years' formal schooling, in the Harris Normal School, in which he later served as principal, from 1880 to 1883, just before he left North Carolina and the South for good. For the most part, he was self-taught—in mathematics, the classics, German, French, and stenography. But it was stenography that later made it possible for him and his family to lead a comfortable, middle-class life, which satisfied most material wants and allowed him to send his daughters to Smith College and his son to Harvard.

In May 1883 Chesnutt bade good-bye to both family and North Carolina and headed for New York City, where for six months he worked as a reporter for Dow, Jones, and Company and contributed a business column to the *New York Mail and Express*. Then he went back to Cleveland, where he spent the rest of his life. By the spring of 1884 he could afford to send for his family, and over the years he became businessman (with his own stenography firm), court reporter, lawyer, author of fiction and nonfiction, and champion of full and equal rights for the black race.

Chesnutt published his first story in 1886, in the *Chicago Ledger*, and then moved on, with other short stories, to *McClure's* and the *Atlantic*; to the *Independent*, among others, with nonfiction; and to Houghton Mifflin and Doubleday, Page, and Company with novels. He wrote nine novels, but only three were published. Chesnutt was a good novelist, so the critics said, even though he never learned, perhaps because he was a master short-story writer, to develop his characters fully. But it was his novelist's concern for social action over literary craftsmanship that deprived him, in an age of growing segregation and other racial proscriptions, of the reading public that he deserved.

Amply supported by his stenographic business and driven by a boundless energy, Chesnutt knew well and corresponded with two white southern intellectual emigrés—George Washington Cable and Walter Hines Page—while he differed with, but respected, Booker T. Washington and had a cordial relationship with W. E. B. DuBois. Never believing in a back-to-Africa movement and always despairing of the white man's acceptance of the black man as an equal, Chesnutt became an "amalgamationist" who truly believed that racial harmony in America would never exist until the two races gradually became a complete admixture. This, of course, caused many to regard him as a dangerous radical. He was not. His life confirmed the status of blacks in the early twentieth century: no matter how far a black man lifted himself, or how gifted and accomplished he was, he had to remain a man apart so long as he was identified as a Negro.

Frances Richardson Keller tells this story simply

and directly, with rich and ample quotations from Chesnutt's papers, unpublished manuscripts and journals, and published works. Brigham Young University Press has produced an attractively designed and printed book—with the footnotes at the bottom of the page where they belong.

CHARLES E. WYNES
University of Georgia

ROBERT PEEL. *Mary Baker Eddy: The Years of Authority*. New York: Holt, Rinehart and Winston. 1977. Pp. xii, 528. \$16.95.

With this book, Robert Peel concludes his three-volume biography of the discoverer and founder of Christian Science. He resumes his account with the removal of the Eddy ménage from Boston to "Pleasant View" in Concord, Vermont (1892) and carries the narrative through to Mary Baker Eddy's death at Chestnut Hill, near Boston, in 1910. These were eventful years during which she restructured her church organization (1892), successfully confronted the threats of such would-be rivals as Josephine Woodbury (1901) and Augusta Stetson (1909), repulsed a much publicized legal challenge to her mental competence (1907), ordered the establishment of the *Christian Science Monitor* (1908), and oversaw the remarkable growth of Christian Science in the United States and abroad.

Peel is a painstaking and imaginative scholar whose laudable aim has been to produce a more balanced picture of his subject than that presented by the gushing Sybil Wilbur or the debunking Edwin Dakin. In several major respects, he succeeds. Access to the Mother Church Archives, which he, as a Christian Scientist, secured, yields revealing anecdotal material invaluable to vital biographical writing. Peel even permits himself a brief excursion into psychobiography. Moreover, he effectively scotches those extravagant contemporary reports of Eddy's physical and mental debility, which for nearly a century have provided the more sensational episodes in the apochrypha of hostile authors.

There are, however, significant problems to be noted here that, in large measure, must be attributed to the author's religious convictions. Since Peel insists upon Eddy's unique, providential role, his treatment is uncomfortably reverential and, except in minor matters, generally uncritical. Special pleading resolves all doubtful points in favor of the subject. Thus, for example, what appears to be a clear (though, perhaps, merely technical) plagiarism is blandly passed over with the startlingly irrelevant observation that the incident serves to show Eddy's occasional published pieces to be

fallible (pp. 107-08). Furthermore, entirely too much is claimed for her acumen in world affairs, and, indeed, her comments and pronouncements on public events were quite commonplace. Of course, Peel's approach almost inevitably precludes any development of the element of real humor inherent in such matters as the squabble over *Christ and Christmas* or Eddy's brief flirtation with the Anglo-Israel myth.

Although the foregoing considerations constitute serious reservations about this study, the work represents an important achievement. No biography is definitive, least of all a biography of so controversial a figure. Still, until scholars of all persuasions are permitted to peruse the archival sources, Peel's volumes must, for sheer extensiveness of documented research, take precedence over earlier lives of Mary Baker Eddy.

RAYMOND J. CUNNINGHAM
Fordham University

PATRICIA T. DAVIS. *End of the Line: Alexander J. Cassatt and the Pennsylvania Railroad*. New York: Neale Watson Academic Publications. 1978. Pp. 208. \$15.00.

Many Americans today would associate the name Cassatt with Mary, the artist, rather than with her brother, the seventh president of the Pennsylvania Railroad. Born in Pittsburgh in 1839 in a family of wealth and influence, Alexander J. Cassatt did not fit the Horatio Alger mold. After graduating as a civil engineer from Rensselaer Polytechnic Institute in 1859, Cassatt worked briefly on railroads in Georgia but in 1861 returned north to work for the Pennsylvania Railroad. Even though Cassatt was a healthy young man in his early twenties, he escaped service in the Civil War. Soon both J. Edgar Thomson and Thomas A. Scott were impressed with his ability and diligence, and he quickly advanced from engineering to positions of responsibility in management. In 1873 he was general manager of all Pennsylvania lines east of Pittsburgh and by 1880 was made first vice-president. During these years he was an advocate of standardization of motive power and early saw the advantages of the Westinghouse air brake. Cassatt also played a leading role in the railroad strike of 1877, the oil rebate negotiations with John D. Rockefeller, and the Pennsylvania's acquisition of the Philadelphia, Wilmington, and Baltimore Railroad in 1881. The entire railroad industry was surprised when in 1882 he retired from active service, retaining only a directorship in the company.

In 1899 after many years of retirement, Cassatt was elected to the presidency of the Pennsylvania, a position he retained until his death in 1906. Dur-

ing these years his engineering expertise cleared up traffic problems and greatly improved the operating efficiency of the entire line. In the seven years of his presidency total traffic increased by half and earnings nearly doubled. As a long-time foe of rebates, he convinced his fellow directors, as well as the managers of other eastern lines, to join together in a "community of interest." Working together, these major eastern trunk lines ended the granting of rebates, successfully challenging Carnegie and other industrialists who had long benefited from these special rates. Perhaps a greater achievement was Cassatt's success in moving the eastern terminal of the Pennsylvania from the west bank of the Hudson to the heart of Manhattan Island. In 1901 he started the \$115,000,000 engineering project that included the tunnel under the Hudson, the Pennsylvania Station in mid-town Manhattan, and extensive terminal facilities on Long Island. While construction was not completed until 1910, under his successor, the major credit for the ambitious project clearly belongs to Cassatt.

Patricia T. Davis has written a lively biography, but several errors of fact have crept into the volume. The author credits Commodore Vanderbilt with owning the Erie, has Gould's partner, Jim Fiske (sic), commit suicide, and suggests that most late nineteenth-century railroads received extensive land grants. Although there is no documentation, the author has used company records, presidential letterbooks for 1899-1906, and family letters belonging to Cassatt's granddaughter. More than two dozen excellent photographs supplement the text. *End Of The Line* is a welcome addition to the history of railroad management in an era well before American railroading went into decline.

JOHN F. STOVER
Purdue University

LEWIS L. GOULD. *Reform and Regulation: American Politics, 1900-1916*. (Critical Episodes in American Politics.) New York: John Wiley. 1978. Pp. ix, 197. \$10.95.

This book is one of a series, edited by Robert A. Divine of the University of Texas at Austin, which grew from a premise that a "resurgence" of interest in political history has occurred in recent years. The title of the present work encapsulates the belief of the author, Lewis L. Gould, also of the University of Texas, that "the interaction of debate over government regulation with the attack on partisanship gave the politics of the Progressive Era its unique character" (p. v). Gould focuses on the way in which the two political parties were affected by this interaction. He treats in some

detail the presidential and congressional elections from 1900 through 1916, discusses the presidential politics of the Roosevelt, Taft, and first Wilson administrations, and puts in the framework of congressional maneuvering those elements of the legislative programs that relate to federal government regulation.

Although Gould's evaluations of the three presidents are balanced, that of Wilson is less persuasive than the other two. He credits Wilson in one paragraph with a mind that was "flexible, adroit, and opportunistic"; then in the next paragraph the author provides him "a tendency toward rigid opinions and intolerance of opposition" (p. 136). He concludes also that Wilson used the Democratic Party for his own purposes (for which the evidence is no greater than for any other aspiring politician) and that Wilson strengthened the party "for a season" and "left it weaker . . . than when he first became its leader" (p. 137). Although such a conclusion is defensible, it does not acknowledge the unprecedented diplomatic and political situations faced by Wilson in the closing years of his presidency, nor does it sufficiently incorporate data, which Gould appropriately mentions, suggesting Wilson's health as an explanatory factor.

Gould writes with authority and detachment. He has an intimate knowledge of the relevant manuscript collections, and his bibliography and footnotes indicate a mastery of recent scholarship. He consciously refrains from making his study one of good against evil and generally discusses rather narrowly the issue at hand, allowing his analysis to speak for itself without referring to the interpretations of others. For the particular subject of government regulation, such an approach has its drawbacks. To understand the development of government regulation, for example, more needs to be said about the evolving structure of industry and about the considerable theorizing in the contemporary professional and popular literature about "the new competition" and about the proper role of government. Although it appears by implication, moreover, that Gould does not fully accept the interpretations of historians such as Gabriel Kolko, who have established distinctive and influential positions on government regulation, Gould's readers might wish to see these interpretations confronted specifically.

Such omissions were, no doubt, required by limitations of space imposed by editor and publisher. To require an author to tell such a story in fewer than two hundred pages poses an impossible challenge of synthesis and condensation. In general, Gould meets this challenge skillfully and has made a particular contribution in his description of the political structure of the early twentieth century, in his analyses of the two political parties, and in his explanation of the origins of the Progressive

Party in 1912. It is indeed refreshing for a book to deal authoritatively with parties and elections and admit the premise of the "enduring importance of political events" (p. vii).

RICHARD L. WATSON, JR.
Duke University

JUDITH A. BAER. *Chains of Protection: The Judicial Response to Women's Labor Legislation*. (Contributions in Women's Studies, number 1.) Westport, Conn.: Greenwood Press. 1978. Pp. x, 238. \$16.95.

Applying scholarship to the battle against what she calls "the universality and injustice of male supremacy" (p. ix), Judith A. Baer has produced a well-researched, closely argued, and interestingly written study of the laws and judicial decisions relating to protection of women workers in twentieth-century America. A feminist and specialist in public law, Baer attempts to combine the examination of the rise and fall of constitutional legitimacy for protective legislation with an analysis of the morality and justice of differential treatment of the sexes.

Chains of Protection traces several phases of protectionism in the legislatures and the courts. Until 1908, laws restricting employment of females were usually rejected under the doctrine of freedom of contract. With *Muller v. Oregon*, however, sociological data and the Brandeis brief convinced jurists that women needed protection. But Baer argues that early decisions were based on the assumption of permanent sexual differences among workers sufficient to justify special treatment for women, whose biological ability to bear children destined them for a social obligation as childrearers and homemakers. Protection had expanded to general labor regulation and other kinds of special laws for women workers until, in 1937, the minimum wage for women was upheld in *West Coast Hotel v. Parrish*. The presumption of inequality, rather than changes in socioeconomic and technological conditions, had become the basis for decisions. By the 1960s laws initially propounded as limitations on employers for the benefit of women workers had become restrictions on employed females. More recently, the impact of the Equal Pay Act of 1963 and Title VII of the Civil Rights Act of 1964 has led to reconsideration of protective legislation, most dramatically in the invalidation of every sex-specific state labor law challenged since 1969.

Despite potential dangers of projecting contemporary feminism backward over the historical record, Baer's treatment of the laws and cases frequently sets them in the social, political, and economic context of their own times and effectively illustrates shifting conditions of judicial interpretation. Some readers may also wish for a fuller

treatment of the changing conditions of labor and of working women, but the problem of working-class women as objects rather than subjects of their own history is more difficult. One looks in vain for their views on the conditions that led to protective legislation and their attitudes toward the laws and court cases. But perhaps Baer should not be faulted for failing to write a book she did not intend to write. Since working-class women have often ignored or rejected feminist positions, the book may be better conceived as a moral feminist look at abstract laws. For that purpose, the concluding two chapters contain a fine summation of the better-reasoned arguments on equality and justice, concentrating on the biological, psychological, and philosophical rather than the political and economic.

Historians and political scientists will find *Chain of Protection* both challenging and enlightening. Feminists will welcome the hard evidence it adds to years of impassioned rhetoric.

SUSAN ESTABROOK KENNEDY
Virginia Commonwealth University

JOHN W. BRIGGS. *An Italian Passage: Immigrants to Three American Cities, 1890-1930*. New Haven: Yale University Press. 1978. Pp. xxii, 348. \$20.00.

The proliferating studies in American ethnic history are due to a complex constellation of factors. There is little doubt that they are the result of the social tumult of the 1960s and the egalitarianism in which ethnicity and new ideological protestations were inextricably intertwined. The new ethnic studies also attest to the rewarding areas for investigation that were long neglected by the American academic community. John W. Briggs's volume derives, with virtually no change, from his doctoral dissertation, and both its conclusions and its perspective will provoke controversy.

The title of Briggs's book is misleading; the title of the original dissertation is more descriptive: "Italians in Italy and America: A Study of Change within Continuity for Immigrants to Three American Cities, 1890-1930." The cities are Rochester and Utica, New York and Kansas City, Missouri. Observing that "scholars have subjected the large settlements [of Italians] in New York City and Chicago to considerable study," Briggs notes that these cities "represent one tradition of Italian settlement," but, in his opinion, there is another pattern. "Many Italians passed up the major metropolises to settle in medium-sized and smaller cities and towns. Here the colonies grew to their fullest expression in the years 1890-1930. This book explores this less-well-covered segment of the immigrant experience" (p. xv).

The hypotheses advanced are flawed: the mean-

ing of the phrase "grew to their fullest expression" is conjectural; the generalization that this aspect of Italian immigration is "less-well-covered" is patently not true: C. W. Churchill studied Italians in Newark; V. Y. McLaughlin, in Buffalo, G. La Piana, in Milwaukee, J. A. Scarpaci, in the Louisiana sugar parishes, and Radin, in San Francisco. There are scores of studies on other medium-sized cities and small towns (see F. Cordasco, *Italian Americans: A Guide to Information Sources* [1978]).

Almost a third of Briggs's study is devoted to Old World backgrounds: emigration from southern Italy, mutual benefit societies in southern Italy, and schooling in southern Italy. If one is delighted to find Briggs rejecting Edward C. Banfield's views, it is incongruously banal for him to repudiate Banfield as an immigration historian (which he is not) and to overlook Banfield's theory of "amoral familism," a necessary key to understanding the social ethos of southern Italian society. Briggs's gratuitous attack on Phyllis Williams and Leonard Covello as unknowledgeable about the premigration culture is undeserved. Williams was intimately familiar with the invaluable work of the Sicilian physician-anthropologist, Giuseppe Pitrè, and Covello's *Social Background of the Italo-American School Child* (1944) was based on long sojourns and study in southern Italy. Briggs's "Bibliographical Note" reveals, at best, an inadequate knowledge of Italian archives, in judiciously used, and, at worst, little or no knowledge of Italian.

Briggs's conclusions about Italian immigrants and their progeny in three cities derive from his view on Italian emigration (his reason for studying Old World backgrounds): the view will not withstand scrutiny. In Briggs's own words: "Italian emigration was a selective process initiated as part of a reaction to the threat of a reduction of a complex status system into one based on simple exchange values associated with the wage-labor system. The emigrants give testimony through their mobility to a deep concern for their future places in society rather than to their immediate poverty. In Italy they had already expressed through their societies and associations bourgeois values such as individual advancement through hard work, thrift, education, acquisition of property, and autonomy in their occupational and economic lives" (p. 272). This may reflect the views of Timothy L. Smith (Briggs's doctoral advisor) but is totally out of keeping with the data available in the work of both American and Italian scholars (for example, Robert F. Foerster, *The Italian Emigration of Our Times* [1919] and Fernando Manzotti, *La Polemica sull'Emigrazione nell'Italia Unita* [1969]).

Clearly, then, Italian immigrants did well in Briggs's American cities. They were "active agents, capable of initiative as well as of accommo-

dation . . . [with] a sense of continuity between past and present"; Italian children "fared as well as their classmates in schooling"; the colonies were "culturally and materially prosperous"; and immigrant children continued "their parents' assimilationist tendencies in the public sector" (pp. 272, 274, 276). Briggs would have us believe that Italian immigrants and their children were spared the dominating realities of a hostile America and the damaging consequences of interethnic rivalry, the Americanization movement, psychological testing, race typologies, eugenics and hereditarian pronouncements, and the debilitating results of conflict and enforced acculturation.

Italians in America have been the victims of continuing tragic irony. In the past they have been subjected to the benevolent ministrations of social workers who studied them in the sociopathology inherent in a matrix of deprivation and cultural conflict and to the probings of psychologists who sought to understand the dynamics of adjustment. And now, in the spirit of a new enlightenment, Briggs has recast the Italian immigrants as major protagonists in an American morality play for which the major themes are redemption and social success. Italian-American academicians who have in the last decade been energetically studying their past will not be angered; for the most part, they will smile and be amused.

FRANCESCO CORDASCO
Montclair State College

JOHN BODNAR. *Immigration and Industrialization: Ethnicity in an American Mill Town, 1870-1940*. Pittsburgh: University of Pittsburgh Press. 1977. Pp. xix, 213. \$11.95.

Industrial society was brought into being in the United States by immigrant labor. Irish, German, Slavic, and Italian workers built a massive productive apparatus that was constructed of iron and steel instead of wood, run on rails instead of rivers, powered by steam, and energized by coal. They experienced privation, insecurity, a fatiguing round of toil, injury, and the risk of death. In the absence of significant public or corporate welfare and in the face of anti-union policy, coal and iron police, Pinkertons, injunctions, and strike breaking by the federal government, they turned inward to develop collective support within their communities.

The immigrant family became a boarding house for fellow countrymen, the kin network an aid society, the saloon a labor exchange, and the neighborhood a basis for the fraternal lodge and the benefit society. The adaptations of cultural traditions to the brutal environment of industrial

capitalism brought the ethnic enclave into being. New national identities were created as much in reaction to working-class experience as in reaction to xenophobic pressures and religious prejudices of native Americans. And, as the work of Herbert Gutman reveals, the values and beliefs of the new ethnic communities became, in turn, sources of resistance to exploitative conditions.

The industrial workers of Steelton, Pennsylvania, a mill town of some 13,000 souls along the Susquehanna River, created their own ethnically based community life in the shadow of blast furnaces and rolling mills established by railroad capital after the Civil War and purchased by Bethlehem Steel Corporation during World War I. The years of greatest expansion around 1890 brought a large influx of blacks and the beginnings of Slavic and Italian settlement.

Well-constructed tables showing the occupational distribution of these groups underscore the familiar point that old-stock Americans tended to hold the better jobs and make a new point that Slavs, not blacks, occupied the lowest positions. Efforts to organize the industry in the early 1890s and again in 1919 were hampered by the failure to enlist these unskilled laborers. After the restriction of immigration, however, the Great Depression gave an irresistible impetus to industrial unionism, and the arrival of the CIO, recalled in a number of personal interviews, mobilized the ethnic enclaves to take over their inheritance. It was a legacy no one else wanted any more.

John Bodnar wastes some effort chasing names through fictive hierarchies to determine imaginary "mobility rates," and additional energy is expended to calculate scarcely more important "persistence rates." The attempt to make something out of this trivia, while ignoring altogether the kind of rates that are significant—profit, unemployment, growth, birth, and death—suggests that the new urban history has some basic lessons to learn from the old economics.

But where the author escapes from faddish social science methodology to read Croatian-language newspapers, interview old timers, assemble materials from corporate and government archives, and trace the city over a long stretch of time, he succeeds in demonstrating a very significant point: "ethnicity was the answer of immigrants and their children to exclusion and working-class status" (p. 144).

Where does one go with this conclusion? One view of the broader implications, shared by the author, is that ethnicity and class are an either/or proposition. Another, developed by Victor Greene in *The Slavic Community on Strike* (1968), is that ethnic community solidarity is the basis for militant class action. The truth does not lie somewhere

in the middle, in a concept of an "eth-class." It lies beyond, in a recognition of the dialectical interaction between ethnic cultures and the conditions of working-class life, a process in which both the cultures and the conditions were transformed.

ALAN DAWLEY
Langhorne, Pennsylvania

CLYDE GRIFFEN and SALLY GRIFFEN. *Natives and Newcomers: The Ordering of Opportunity in Mid-Nineteenth-Century Poughkeepsie*. (Harvard Studies in Urban History.) Cambridge: Harvard University Press. 1978. Pp. xvi, 291. \$18.50.

At first glance, Clyde Griffen and Sally Griffen appear to have constructed a competent, thorough mobility study that confirms much of what social historians have discovered about late nineteenth-century American cities. The Griffens examine Poughkeepsie, New York, a relatively placid place that grew slowly between 1850 and 1880 and contained few immigrants. Nevertheless, Poughkeepsie's patterns of social and geographical mobility resemble those of other contemporary cities: rapid population turnover from in- and out-migration; few rags-to-riches success stories; frequent small steps up the occupational ladder and fewer downward than upward shifts; and disadvantages for certain groups, chiefly blacks and unskilled foreigners. Identification of these patterns and differentiation among them by ethnic, racial, and religious subgroups are valuable enough, but the Griffens offer patient readers more, much more.

Unlike most previous mobility studies, *Natives and Newcomers* explains how the process of mobility worked in a particular setting. It probes beyond career mobility, generational mobility, and the like to uncover the upward and downward paths that certain types of individuals took. The reader receives humane, empathetic analyses as well as stock recitations of percentages. Thus we learn that immigrants could best achieve desired self-employment statuses by rising from skilled crafts, particularly in the food preparation and clothing trades. By contrast natives predominated in expanding skilled occupations such as construction and industrial trades, where self-employment (and thus mobility from employee to proprietorship) was less common. Natives also used clerical and sales positions as springboards to business proprietorship far more frequently than immigrants did. As a consequence, social mobility left immigrants in smaller, more precarious positions while native whites gained more security and influence in larger, more stable establishments.

The Griffens refine their analyses even further to explain what the chances of mobility were for indi-

viduals in discrete occupations. For example, they present a detailed discussion of opportunities for artisans in which they show how trades like cabinetmaking and shoemaking were superseded by factory production that either pushed cabinetmakers and shoemakers out of their trade or relegated them to repair work. Carriagemakers, on the other hand, could adjust by transferring their smithing and painting skills to other industries and thereby increase opportunities for self-employment. Thus, although ethnic origin had some influence over a person's original occupation, more important was that occupation's effect on the person's subsequent occupational and mobility patterns.

Throughout, the Griffens impress the reader with evidence of how frequently occupational change occurred and how uncertain many occupations were. Using the fascinating credit reports of R. G. Dun and Co., precursor to Dun and Bradstreet, the authors show that it was relatively easy to start and to lose a business. Of over fifteen hundred firms examined between 1845 and 1880, 32 percent failed to last three years and only 14 percent lasted twenty years or more. The Dun reports add a human dimension, illuminating the feats and foibles of people striving for security and success. In a perspective never before included in a mobility study, the reader also is able to see the precariousness and stunted opportunities of women's occupations.

The book includes other unique features, such as extensive tracings through city directories enabling a longitudinal view of individuals over time that only a few historians have attempted and age distinctions in mobility patterns that show different opportunities for the young and the old. This work is ambitious, yet the authors are careful to qualify weak evidence and to note speculative conclusions. It is also a book to be savored, one that gives personality and meaning to the numbers.

HOWARD P. CHUDACOFF
Brown University

JAMES C. MOHR. *Abortion in America: The Origins and Evolution of National Policy, 1800-1900*. New York: Oxford University Press. 1978. Pp. xii, 331. \$12.50.

In recent years subjects relating to human sexuality have become far more prominent in public discourse than they were before World War II. The historical profession has not been immune to this trend, and books, papers, and symposia on the history of one aspect or another have become commonplace. The one before us is a thorough and dispassionate analysis, primarily of legislative and legal changes, of one of the most vociferously de-

bated parts of this subject now in the public arena. The author is to be congratulated for the careful and objective manner in which he has set before us the historical background of a hot political issue arousing intense emotional reactions. The story is full of ironies—not the least of which is the common assumption today of the “pro-life” groups that they are defending the ancient traditions on which our country was founded against the immoral attacks of those they pejoratively label as “pro-abortion.” For, as James C. Mohr shows, the national policy toward abortion that prevailed during the first half of this century, which has now been overthrown by the Supreme Court, was created during the nineteenth century largely by pressure from the medical profession, with the aid, rather late in the day, of the anti-obscenity Comstock crusade.

During the first twenty years of the nineteenth century, there were no statutes relating to abortion in the American states, and the use of cures for “obstructed menses” (that is, abortifacients) by unmarried women met with considerable sympathy. Medically, there was no proof of pregnancy before “quickening,” and from the contemporary legal point of view the fetus (if any) was not a living person. This situation gradually changed, beginning with the first statute relating to abortion passed in Connecticut in 1821. Despite the new laws, Mohr concludes that there was a very substantial increase in the number of abortions in the middle decades of the nineteenth century and that the practice was widely used by married middle-class women as a means of birth control. The chief opposition and agitation for further legislation came from organized medicine, spurred by Dr. H. R. Storer of Boston. The physicians were motivated, Mohr suggests, not only by the belief that abortion was morally wrong, by nativism, and by conservative antifeminism, but also by professional considerations. Openly at least, abortions were largely practiced by irregulars and quacks, and this gave the profession an excellent opportunity to attack their rivals while defending themselves on unimpeachable grounds. The religious press, however, very generally ignored the issue until supporters of Anthony Comstock after the Civil War began attacking abortionists’ advertising as obscene. In state after state, under heavy lobbying from medical societies and with no significant opposition, legislatures from 1860 on passed laws making abortion (without reference to quickening) a crime, for both the operator and the woman. The general situation created by 1900 remained essentially stable until the 1960s.

Mohr’s evidence for the primary role played by organized medicine in this movement is convincing, although the lack of opposition after mid-

century suggests the existence of widespread if silent support. Moreover, silent opposition from the middle class may be presumed to have declined as other methods of contraception came to supersede abortion, which again became the last resort of the poor and unfortunate. Mohr’s exploration of concomitant social, professional, and, to a lesser extent, scientific aspects of changing legislation and legal interpretations relating to abortion is illuminating. His work is well documented, considering the nature of the subject. It may be recommended to those involved in current policies as well as to those whose interests are purely historical.

JOHN B. BLAKE
*National Library of Medicine,
Bethesda, Maryland*

DOONE WILLIAMS and GREER WILLIAMS. *Every Child a Wanted Child: Clarence James Gamble, M.D., and His Work in the Birth Control Movement*. Edited by EMILY P. FLINT. (Countway Library Associates Historical Publications, number 4.) Boston: Francis A. Countway Library of Medicine; distributed by Harvard University Press. 1978. Pp. xvii, 445.

An heir to the Ivory Soap fortune, Clarence J. Gamble took up what he called “The Great Cause” of birth control in the 1920s. From that time until his death in 1966 Gamble played a pivotal, if sometimes ambiguous and frustrating, role in advancing that cause. He was instrumental in establishing many of America’s pioneer birth control clinics; he helped persuade public health officials to accept birth control as a legitimate medical concern; he invested money, often at critical junctures, to develop and test various contraceptive methods and substances from brine to the pill and the IUD; and he sent what amounted to private missionaries to Puerto Rico, Asia, and Africa to preach the gospel of birth control on a global scale. The previously neglected details of these and other turning points in the history of birth control are certainly worth having in print.

Gamble was a puzzling and contradictory character: extremely intelligent but insensitive; holder of the M.D. degree and of university appointments but quite profoundly unscientific; perversely defensive about his great wealth yet frequently insistent upon exercising even the tiniest shreds of power his money made possible; a conservative Republican on remarkably good terms with the sometimes radical Margaret Sanger; and one of the world’s most prominent philanthropists in the field of birth control but ultimately driven from the board of the International Planned Parenthood Federation by co-campaigners who came to loathe

him. These fascinating puzzles are mentioned or alluded to in this study rather than addressed analytically. Little is made of the apparently strained relationship between this defender of planned parenthood and his own children, and we get only the most conventional generalities about the way this champion of wives the world over treated his own. In short, this biography ultimately fails to come to grips with its subject.

Much of this may be explained by the fact that this book is what is commonly known as an authorized biography. While the Countway Library at Harvard lent its prestigious imprimatur to this volume, publication was underwritten by the family foundation that Gamble himself established, and copyright is held by his widow. A footnote in James Reed's recent study of the birth control movement in America indicates that the original manuscript for the biography under review here was approximately twice as long as the published version. It seems likely that some of the more analytical passages of the original, which would necessarily be unflattering to Gamble at various points, were excised from the final edition. The Gamble family acknowledges an editor, Emily P. Flint, on the title page along with the co-authors.

Whoever revised the manuscript did not do an especially skillful job. There are many awkward transitions, and some of the paragraphing would not pass muster on an undergraduate paper. The final frustration in this biography is its lack of footnotes. There is no particular reason to doubt the book's factual veracity (the random points I was able to check in other sources were all accurate and the Gamble papers are open to investigators), but it will certainly make more difficult the job of those scholars who wish to get behind the tantalizing narrative offered here to the still unexplained riddles that surround the historically significant figure of Clarence J. Gamble.

JAMES C. MOHR
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ROBERT A. WALLER. *Rainey of Illinois: A Political Biography, 1903-34*. (Illinois Studies in the Social Sciences, number 60.) Urbana: University of Illinois Press. 1977. Pp. xii, 260. \$12.50.

The period 1903-34 represents the nadir of the Democratic party in the United States. Only in 1930 did the Democrats begin to emerge from their minority status. In Illinois, where the party was even more dejected, the Democrats usually elected only about 20 percent of the congressional delegation, mostly from ethnic neighborhoods in Chicago. Henry T. Rainey, Democrat, represented a

rural, downstate district in Congress for the entire period, save one term from 1921 to 1923, serving as speaker during the hectic early days of the New Deal. In the chaotic political scene of Illinois, Rainey supported the mildly reformist leadership of Mayor Carter Harrison in opposition to boss Roger Sullivan. His career was not based on patronage because, even when the Democrats were in a position to dispense jobs, he often was at odds with the leadership.

This remarkable career demonstrates that Rainey was a congressman with considerable political skills, but he also had statesmanlike qualities that made him keeper of the liberal conscience during the 1920s. Essentially a populist, Rainey was suspicious of the cities and was noted as a spokesman for agrarian reform. Rainey linked the farmers' plight with the high tariff policies of the Republicans and opposed the McNary-Haugen bill. As speaker, Rainey had a part in passage of the Agricultural Adjustment Act. Rainey promoted the concept of a deep waterway from Chicago to New Orleans. Like all good populists, he opposed the special interests, monopolies, high tariffs, and regressive taxes. In the Democratic convention of 1924, he voted dry and opposed the Klan.

An internationalist, Rainey supported the building of the Panama Canal; but after visiting the Canal Zone at his own expense, he denounced the corruption he found in its construction. Increasingly, he criticized the manipulations of Theodore Roosevelt in acquiring the Canal Zone. He bitterly attacked the close association of Roosevelt and William Nelson Cromwell, the New York lawyer who made many of the behind-the-scenes arrangements. Rainey characterized Cromwell as "the most dangerous man" since Aaron Burr. At a time when Americans still disagreed about the wisdom of recognizing the Soviet Union, Rainey traveled to Russia, again at his own expense, and advocated diplomatic recognition. Among other things, he saw Russia as a potential market for American goods.

Robert A. Waller believes that Rainey's impressive career has received little notice because he was part of a dispirited and submerged party for most of his career. Waller proposes to remedy this neglect with this book, a good overview of an important life. The reader may wish for a more detailed account of portions of Rainey's life. It would be interesting, for example, to know where he went in Russia, what he saw, and to whom he talked. Still, Waller has made a valuable contribution to the understanding of a neglected figure.

DONALD F. TINGLEY
Eastern Illinois University

PATRICK J. MANEY. *"Young Bob" La Follette: A Biography of Robert M. La Follette, 1895-1953*. Columbia: University of Missouri Press. 1978. Pp. 338. \$18.00.

Robert M. La Follette, Jr. was the first to agree that he was not a chip off the old block. His brother, Phil, readily confessed to that role. It complicated the lives of both since Phil was the younger, not the anointed heir, and too young to run for their father's Senate seat when Old Bob died in 1925.

Being a La Follette was a consuming occupation. The family set great store by their oratory, welcomed advice and criticism only from the family circle or devoted disciples, and maintained the circle like threatened musk oxen. Robert, Jr. did what was expected of him by the family. He possessed gifts of a high order. His problem was that he did not like elective politics. Phil did, but was temperamental, impulsive, and no great strategist.

The main outlines of Patrick J. Maney's able biography and his conclusions are not particularly new or startling. He has no serious arguments with Roger Johnson's earlier, slighter, and more limited work (*Robert M. La Follette, Jr. and the Decline of the Progressive Party in Wisconsin* [1964]). Why, then, is a second biography warranted? Phil, who was working on his uncompleted memoirs in the State Historical Society of Wisconsin, complained that he could use neither his father's nor his brother's papers, which were under the control of his older sister Fola, even though he had trading stock in his own papers, which he was ready to share. Fola outlived him by several years. It seemed the natural antithesis of the unusually close family relationship during the active political years.

Maney, with the advantage of a Wisconsin background, did his graduate work at Maryland, handy to the Library of Congress where the La Follette papers were opened to scholars in 1970. Maney's research included this huge family collection, plus other leads found there, and the usual material open to earlier scholarship, conducted mostly from Madison. He has done an admirable job, adding greater depth to the available portraits of Old Bob, his two sons, and their mother, Belle. If it was necessary, Maney also established Young Bob's important role in politics and government at the national level from the time he outmaneuvered President Coolidge on the "I do not choose to run" statement to the disastrous Wisconsin primary of 1946. Young LaFollette especially attracted the notice of Washington reporters and pundits during the New Deal years, figured on short lists of possible successors to FDR, and was as close to a recognized leader as the senatorial progressives would tolerate, despite the disadvantage of knowing Norris and Borah as "Uncle George" and

"Uncle Bill" a few years before while he was still in knee pants. He was the scholar in politics, the Senate's acknowledged tax expert, a man of conscience, and the one who best knew the customs and rules and how to use them. He was, in contrast to his father, well liked there.

Although he enjoyed doing the public's business, he disliked tending political fences. He was the victim of a father who demanded total devotion and was a master of emotional overkill. When Young Bob, out from under the parental eye for the first time, was sent to the University at Madison, his successes and failures registered on the family seismograph in Washington. When his grades put him on the probation list, his father required a daily report "on any class recitation or examinations." Mrs. La Follette was dispatched to the scene, where she sat in on a class, quizzed Robert's fraternity brothers, interviewed his professors, the dean, and the president. One gets the impression that this overwrought concern had to do with the young man's future political prospects as much as anything.

Six years before the elder La Follette's death, he was writing home: "When the last night comes and I go to the land of No Return, what an awful account of *things undone* I shall leave *behind*" (p. 30). He unloaded this on his son and namesake, who had the misfortune to follow his sister, Fola, thirteen years after her birth. The father had anxiously awaited his arrival. Bob had frequent long bouts with serious illnesses from his teens and was not up to his father's demands, physically or emotionally. Maney develops this without pretense at psychobiography. You won't find it elsewhere. The book is a real contribution, with a valuable bibliographical essay.

ROBERT C. NESBIT
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Madison*

RAYMOND FIELDING. *The March of Time, 1935-1951*. New York: Oxford University Press. 1978. Pp. viii, 359. \$14.95.

The *March of Time* has always been something of an enigma in film history. Given its general unavailability and the lack of detailed study of it, Raymond Fielding points out that many misconceptions have arisen about the series—especially concerning its sociopolitical orientation. Fielding's careful research and well-written history of the *March of Time* should both correct these misconceptions and encourage further investigation of its role in the years before, during, and after the Second World War.

Combining detailed analyses of the films them-

selves, material contained in production files, press reactions of the time, and extensive interviews with virtually all of the principals involved in the productions, Fielding has woven a highly readable text that presents a wealth of information and observations. He sets the stage by briefly outlining the development of the newsreel, the introduction of *Time* magazine, and the success of the radio version of the *March of Time* and of Columbia's *March of the Years*. He describes the efforts of *Time*'s general manager Roy Larsen (with the assistance of Fox Movietone's Louis de Rochemont) in translating the radio version into film. Fielding attributes the distinctive character of MOT to de Rochemont since his domineering personality allowed for precious little innovation on the part of MOT staff. Although there are obvious parallels here with John Grierson's position in the British documentary movement of the 1930s and the National Film Board of Canada, de Rochemont's control seems to have been more complete and less sensitively applied. Indeed, Fielding contends that much of the reason for MOT's demise was its failure to continue to innovate.

Much of Fielding's volume is personality-oriented; in fact, each of the principals of the series is discussed at some length. Other foci include important individual issues and feature-length productions, MOT's relationship to the documentary film, the series' methods of operation, critic and audience response, its role in World War II, and its eventual decline and fall. Fielding also provides bibliographical notes (in lieu of footnotes), an extensive bibliography, a description of additional research resources, a complete list of MOT episodes and credits for longer productions, and a list of awards won. This information should prove invaluable to anyone wishing to pursue further study of the series.

Although intended as a history, this book is also a timely and relevant offering. "The *March of Time*," Fielding writes, "was neither documentary, nor newsreel, nor dramatic product, and yet it had elements of each" (p. 77). In attempting to illuminate this statement, Fielding spends considerable time throughout the book describing MOT's style and approach. He relates incident after incident of the combining of actuality footage with re-enacted, pre-enacted, and "provoked" material. These descriptions and discussions provide a historical grounding for the consideration of the issue of "docudramas" that have begun to appear on television screens in recent years, as Fielding acknowledges in passing references to the current trend.

Fielding's book, then, is both interesting and informative. There are, however, some grating aspects to it. The failure to use footnotes (although references are integrated into the text, leading to

easy reading, and are supplemented by bibliographical resources at the end) leads to an unsettling sense of *déjà vu* when Fielding borrows from his earlier *American Newsreel, 1911-1967*. He also exhibits an overly reverential attitude toward the individuals involved, perhaps borne of his long involvement with these people. More disturbing, however, is his rather violent tirade against *cinéma vérité*, which seems quite unnecessary—or at least overdrawn—for the purpose of justifying a more formative approach to recording reality. There is also a need for a fuller investigation of the relationship between MOT and the film audience. Measured against the book's successes, however, these problems are rather minor. *The March of Time, 1935-1951* will be useful for those who wish to expand their sense of the factual film in this important period of its history.

JAMES M. LINTON
University of Windsor

NICK A. KOMONS. *Bonfires to Beacons: Federal Civil Aviation Policy under the Air Commerce Act, 1926-1938*. Washington, D.C.: U. S. Department of Transportation. 1978. Pp. vi, 454. \$8.00.

Nick A. Komons maintains that a community of businessmen acting alone could not have realized the airplane's potential and that the government was civil aviation's "indispensable partner" (p. 3). As the Federal Aviation Administration's resident historian, Komons might be expected to take such a position, but he marshals impressive evidence to support his contention. Perhaps most compelling is a 1921 Herbert Hoover quotation: "It is rather startling, to say the least, to have an industry . . . asking and urging legislation putting the business completely under Federal control" (p. 22). Even more remarkable, the comatose Coolidge administration responded to the plea by masterminding the Air Commerce Act of 1926, assigning authority to Hoover's Commerce Department. The act was designed to foster civil aviation as well as regulate it, and the Republican administrations of Coolidge and Hoover, with the latter playing the leading role in both, emphasized promotion.

Komons argues convincingly, however, that regulation furthered aviation's development more than promotion did. And, although Hoover justifiably took pride in bringing order out of the "chaos of laissez faire," his administration's collusion with the large airlines over financially vital air mail contracts made a high bankruptcy rate among small operators one price of order.

The book forcefully depicts the continual frustration of coping with rapid technological change. Beacon lights to permit night flying and radio to

facilitate all-weather flying were such monumental breakthroughs that the reader wonders how fliers once survived without them. Yet aircraft advances quickly created new problems, and federal aviation personnel were constantly forced to cope with congressional lag—funding for new navigation systems was always a few exasperating years behind perception of the problems.

Roosevelt's talk of the need for a coordinated national transportation system momentarily raised the hopes of the aviation community, but the preoccupied president did not share their sense of urgency. The total federal aviation budget for fiscal 1934 came to only \$7.6 million; in contrast, the Civil Works Administration spent \$11.5 million priming the pump with ill-planned airports that winter. The investigation following the air death of Senator Bronson Cutting in 1935 glaringly exposed the inherent conflict of interest in Bureau of Air Commerce self-investigations, yet not until 1938 did FDR finally support new aviation legislation and provide some direction for the tortuous congressional intrigue over reform. The resultant Civil Aeronautics Act of 1938 established an autonomous Civil Aeronautics Authority that Komons calls "the most complex [organization] ever created by the Congress" (p. 379). Civil aviation had come of age, but, far from meaning independence, maturation meant ever-increasing reliance on a strong federal role.

This is the second of four FAA-commissioned volumes on the agency and its predecessors (Stuart Rochester's *Takeoff at Mid-Century: Federal Civil Aviation Policy in the Eisenhower Years, 1953-1961* was reviewed in the *AHR* in December 1977). Like the first, it is very well written; Komons has a gift for integrating quotations smoothly into his text. He does betray a narrow focus when he claims that the Air Commerce Act was "perhaps the only genuine legislative achievement of the Coolidge Presidency" (p. 88) and that the addition of miles of lighted airways was "perhaps the only triumph that came out of the ruins of [Hoover's] administration" (p. 144). Nevertheless, the volume admirably recounts the early struggles in the growth of federal regulation of civil aviation, exhaustively documents the research involved, and gives readers the definitive treatment of the subject.

JOHN R. M. WILSON
Mid-America Nazarene College

JULES ROBERT BENJAMIN. *The United States and Cuba: Hegemony and Dependent Development, 1880-1934*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1977. Pp. xii, 266. \$14.95.

This work, a highly analytical study of United States-Cuban relations, focuses on the years from

1925 to 1934. In these years the despotic rule of Gerardo Machado, whose coming to power in 1925 portended a new era in Cuban history, was systematically assaulted by reformist elements that struggled for the creation of a "New Cuba" and a different relationship between the island and the United States. The 1933 revolution symbolized what this struggle meant; in that year the most radical and the most anti-American groups triumphed briefly in the aftermath of Machado's downfall. The Roosevelt administration, represented in Havana by Sumner Welles, sensed the threat the Ramón Grau San Martín government posed for American hegemony in Cuba. The United States undermined the Cuban revolution, not by choosing another Machado but by the expedient selection of a more moderate Cuban nationalist, Fulgencio Batista y Zaldívar. Though it officially repudiated intervention, the United States nevertheless continued its dominance over Cuban affairs long after 1933.

Knowledgeable students of Cuban-American relations will recognize several of Jules Robert Benjamin's major themes as updated versions of views that have previously appeared in the works of Robert Freeman Smith, Irwin Gellman, and Luis Aguilar. Benjamin's contribution lies in the intensive probing of the Cuban-American relationship in the economic crisis of the early 1930s. From the 1880s, he argues, the American economic presence in Cuba made the island increasingly dependent on the United States. The long wars against Spanish rule decimated the native aristocracy and left the island with no landed patrician class to resist American economic intrusion. By the early 1920s, when the sugar boom ended, Cuba's elitist elements, reared in the shadow of American power, were unable to resist further intrusion of American economic forces. In the early 1930s, when the sugar economy collapsed, the United States deserted the old Cuban elite and channeled its energies toward dealing with the anticolonialist and anticapitalist forces spawned in the last years of the Machado dictatorship.

Benjamin devotes almost half of his book to the New Deal and its handling of Cuban affairs. In interpreting the Good Neighbor policy in Cuba, Benjamin implicitly renders to Roosevelt his grudging admiration for its skill in "co-opting" the Cuban revolution and preserving American hegemony. In virtually all of the earlier studies of the 1933 revolution, Sumner Welles and Fulgencio Batista occupy the center stage of events. But to Benjamin the explanation for the course of events lies in the structure of the Cuban economy and the island's dependence on the United States.

Though he writes in a somewhat ponderous style, Benjamin has produced a solid account, based on official records (not only from the State

Department but also from the Departments of Agriculture, Treasury, and Commerce) and private papers.

LESTER D. LANGLEY
University of Georgia

JAMES K. LIBBEY. *Alexander Gumberg and Soviet-American Relations, 1917-1933*. Lexington: University Press of Kentucky. 1977. Pp. xiv, 229. \$13.50.

For nearly fifty years Alexander Gumberg's role in Soviet-American relations during the non-recognition period has remained obscure; James K. Libbey's study has removed him from the shadows and established for him a significant place among those who kept the spark of interest in the recognition of Russia alive. Libbey's was not a simple task, for much of Gumberg's personal life had to be assembled from fragmentary evidence. He had more to say in his correspondence concerning his life's work—reconciling his native and adopted lands—than of his life per se.

Libbey will be challenged on some of his assertions. He is the first to defend U.S. Ambassador to Russia David R. Francis and may possibly be the last. Russian scholars may find fault with his failure to use the Library of Congress system of transliteration for Russian personal and place names. He pays little attention to Albert Fall's efforts to aid Harry F. Sinclair's grab for Soviet oil concessions, which accounted for Warren Harding's interest in Russian recognition at least as much as Gumberg's proddings. Libbey concludes that the recognition effort "collapsed" in the Coolidge years and did not revive until "the full weight of the depression and Democratic administration came in" (p. 116). This ignores the persistent efforts of Henry L. Stimson to use Russian recognition as a block to Japanese expansion. The "four circumstances" that Libbey lists as responsible for making Russian recognition possible in 1933 should have been five: the Far East crisis provided an impetus and catalyst for recognition and ought to have been included. Several questions concerning Frances Adams Gumberg seem to demand answers. How many doors were opened to Gumberg by his marriage to a descendant of John Adams? Did she have anything to do with his Republican leanings? How deeply was she involved in his lobbying efforts?

Whatever the shortcomings of this study, they are far outweighed by its contributions. Most students of Soviet-American relations have had a distorted view of Gumberg and have underestimated his importance, at least in part because of John Reed's biased portrait, but also because of the official suspicion of him in Department of State

records. Both of these biases are now put to rest. Libbey has expressed clearly the crux of Soviet-American antagonisms, which rested on mutual disrespect and suspicion as early as 1917 (p. 27). Despite the author's exaggeration of Gumberg's importance in some instances, there is no longer any doubt that he was a crucial agent in Soviet-American relations. He was a major force in keeping the issue of recognition constantly before the government and the public in the period when so many forces worked against an American initiative toward establishing diplomatic relations with the Soviet Union.

Libbey convincingly describes both Gumberg's obsession with Soviet-American understanding and his motives, which rested on a combination of his Russianness and love for his adopted country. Like Raymond Robins, Gumberg foresaw great problems ahead for both countries if they did not learn to respect one another. Libbey refers frequently to Gumberg's detachment from ideological identifications, undoubtedly the reason why Gumberg lasted the course and was influential in both countries. One is reminded of Maxim Litvinov's request after Bullitt left the Soviet Union to send no more idealists; he preferred a hard-headed American businessman with no illusions to be destroyed. Gumberg loved Russia and America, not communism and capitalism. This volume will be cited often by those searching out Soviet-American contacts between 1917 and 1933.

EDWARD M. BENNETT
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JOHN H. BACKER. *The Decision to Divide Germany: American Foreign Policy in Transition*. Durham, N.C.: Duke University Press. 1978. Pp. x, 212. \$9.95.

This is an intelligent and, for the most part, well-reasoned book on American diplomacy and Germany from 1943 to 1947. But it adds extremely little to our knowledge; the story the author tells has been told several times before and told better.

Backer claims that both traditionalist and revisionist accounts of this period agree in seeing a grand design in American policy. His view deviates greatly from this commonly held belief, he says, because it stresses that there was no overall policy for Germany and that American strategy was the product of "disjointed incrementalism." But whatever one thinks of "history is just one damn thing after another" as an explanation, it is not a new gambit in tracing the origins of the Cold War in Germany: it is the framework John Gimbel uses in *The Origins of the Marshall Plan* (1976).

Moreover, *The Decision to Divide Germany* is not true to the explanatory device the author adopts.

In conflict with "disjointed incrementalism" is his attempt to show that "national belief systems," "historical perception," and "the selective screen of . . . society's value and image system" were crucial determinants of United States policy. The author does not handle these fashionable social science concepts well, but his occasional intent is to argue that behind the seeming chaos of events there is a minimal order that can be understood by the careful historian interested in the world view of diplomats; but the author never works this problem out in a cogent way.

Depending on their own proclivities, readers interested in these topics are still advised to examine Herbert Feis's work on the German question or the studies by revisionist writers who have written on the topic.

BRUCE KUKLICK
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Executive Sessions of the Senate Foreign Relations Committee: Eighty-Third Congress, First Session, 1953. (Historical Series, number 5.) Washington, D. C.: Government Printing Office. 1977. Pp. vii, 870. \$6.85.

This volume covers approximately the first six months of Dwight Eisenhower's presidency. The Korean War still ground along, Joe McCarthy was riding high and recklessly, and John Foster Dulles had been unleashed to convert the heathen. Connoisseurs of the Cold War will find much vintage material here: abroad all strings were pulled from Moscow, while at home the threat of subversion lurked everywhere. On the latter issue, the sexual preferences of State Department employees consumed a good deal of time at several hearings. "All homosexuals are sex deviates, but all sex deviates are not homosexuals?" asked Senator Charles Tobey of a departmental official (p. 88). It does not require many pages of discussion on this level to induce drowsiness in the reader.

The most interesting sessions were the appearances before the committee of French Minister of Defense René Pleven, Chairman of the Joint Chiefs of Staff Omar Bradley, Under Secretary of State Walter Bedell Smith, Ambassador-Designate (to Russia) Charles Bohlen, and Dulles himself. Referring to French Indochina, Pleven assured his listeners that the "agitation" there was not for independence but for a "Communist regime" (p. 295). Speaking to the same subject, Dulles revealed that the "aim of the Kremlin" was to continue the fighting because "by the use of satellite second teams they are able to tie up the first teams of the Western World" (p. 140).

In another appearance Dulles brought out the

domino set. Indochina was critical because if it fell to the Communists "the next operation may be in Northern Malaya or Burma or both." He concluded with the prophecy that after all Southeast Asia went under it would be very difficult "to try and hold Japan" (p. 385). This reviewer found particularly chilling Bradley's almost casual reference to using tactical atomic bombs in Korea if we "could get them [the Koreans] out in the open" (p. 114). This volume is less significant for what it tells us about any particular policy decision than for what it reveals about assumptions held at the time.

ROBERT JAMES MADDIX
Pennsylvania State University

RALPH B. LEVERING. *The Public and American Foreign Policy, 1918-1978*. New York: William Morrow, for the Foreign Policy Association. 1978. Pp. 192. Cloth \$7.95, paper \$3.95.

The frequency with which American statesmen invoke public opinion to explain or excuse their decisions has made the impact of popular views upon the foreign policy of the United States a perennial subject for debate. Ralph B. Levering's book is a survey of those critical periods during the past sixty years around which the debate has revolved: the end of World War I, when President Wilson failed to persuade the Senate to ratify the Covenant of the League of Nations; the late 1930s, when FDR cautiously tried to provide aid for Britain and China against Germany and Japan; the end of World War II and the establishment of the United Nations Organization; the early years of the Cold War; the delayed rapprochement with China in the late 1960s; the era of the Vietnam War; and "detente."

At each juncture the author assesses the popular mood, mainly by reference to opinion polls, editorial comments, and the publications of pressure groups. He finds that education, media usage, ethnicity, and party affiliation are the main determinants of foreign policy attitudes. Economic status, religion, and regionalism are considered less important. Next, he looks at the impact of these opinions upon the decisions of national leaders. Wisely, he endorses neither the view that the "power elite" can always manipulate public opinion to support government policy nor the naively democratic view that public opinion necessarily defines the outer limits of possible foreign policy choices. Instead, he distinguishes those circumstances in which public opinion is most susceptible to leadership from those in which it is not.

He finds that attitudes toward particular countries are most easily manipulated, citing, *inter alia*,

the abrupt change in Russia's image conveyed by American leaders between 1944 and 1947. Attitudes toward American participation in war are, according to Levering, less susceptible to manipulation. No amount of government propaganda can disguise the impact of war on daily life when this involves the draft, battle casualties, and higher taxes. Useful though this distinction may be, it tends to underestimate the sheer force of events upon mass opinion and the reserves of patriotism or xenophobia upon which a president can draw in wartime to gain support for the most obscure foreign causes.

The book's strengths are its clear narrative style, its synthesis of events and trends over more than half a century, and, not least, its well-chosen selection of photographs and cartoons. Levering avoids social science jargon and the kind of resounding invocations of public opinion that George Gallup contributes to the foreword. The professional historian, political scientist, or political psychologist may, however, be disappointed by the familiarity of the events and interpretations on which the author draws. Reliance on secondary literature is not necessarily a defect if the author's insight and imagination cast new light upon old material. Unfortunately, this is rarely the case in an otherwise admirable book.

MICHAEL LEIGH
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BARRY B. HUGHES. *The Domestic Context of American Foreign Policy*. (A Series of Books in International Relations.) San Francisco: W. H. Freeman and Company. 1978. Pp. xii, 240. Cloth \$14.00, paper \$7.00.

This intelligent, well-written survey of a highly complex problem should be read by all students of United States diplomatic history, especially those with an interest in the period since 1945. Until the publication of Barry B. Hughes's book, historians concerned about the way in which behavioral scientists deal with the opinion-policy nexus have had to turn to Gabriel Almond's *The American People and Foreign Policy* (1950), a laudable but outdated work. Hughes covers much of the same terrain but without the anti-Communism that marred Almond's pioneering monograph. More importantly, he is able to analyze a wide variety of new concepts as well as the rich data-based literature that has developed in this burgeoning field over the past thirty years.

Without cluttering his material with social science jargon that intimidates humanists, Hughes describes models of the opinion-policy relationship, the range of opinions on different issues

among the public and policy makers, the roles of parties, the press, and interest groups, and how all of these elements impinge upon the decision-making process. He illustrates his arguments with relevant tables and charts as well as descriptions of prominent historic cases such as the Marshall Plan, the wars in Korea and Vietnam, and the question of recognizing the People's Republic of China. Most of his examples, and indeed most of his analyses, relate to contemporary foreign relations. Allusions to pre-World War II cases are rare, a product both of the lack of polling data for earlier periods as well as the implicit notion that the opinion-policy relationship may be different today from what it has been in the past. Nevertheless, his presentation has applicability for all of American diplomatic history, if only to highlight the complications inherent in any study of public opinion and foreign policy.

Hughes concludes with a rather interesting section in which he demonstrates that no single model fits all foreign policy situations. Depending upon the type of issue (security, diplomatic, or economic), the area of government in which a decision is made (executive or legislative), and the length of decision time, any one of several models of the opinion-policy relationship may be relevant. Thus, he finds different situations that support the validity of classical democratic, rational actor, organizational-institutional, bureaucratic-political, power elite, and pluralist models. This eclectic and not very heroic summation will be disappointing to radical critics of American foreign policy, but it makes a good deal of sense.

Although there is little in Hughes's text that is new for political scientists, diplomatic historians will find it a painless way to learn about what has been going on across the disciplinary boundary over the last scholarly generation. At the very least, he refers to hundreds of recent studies, often from journals historians do not read, that should be of utility to those trying to grapple with the elusive role of public opinion in the foreign policy process, whether in 1812, 1898, or 1964.

MELVIN SMALL
Wayne State University

DAVID CAUTE. *The Great Fear: The Anti-Communist Purge Under Truman and Eisenhower*. New York: Simon and Schuster. 1978. Pp. 697. \$14.95.

If, as David Caute observes in the introduction to *The Great Fear*, America's post-World War II red scare was far milder than the savage repression practiced by the Soviet Union, he also shows that it was nevertheless extensive and chilling, especially in a country whose leaders continually pro-

claimed their devotion to the principles of liberty and democracy. Indeed, the most important virtue of this volume is that it forces the reader to confront the great sweep of the purge and its impact on millions of Americans: the loyalty oaths, the congressional investigations, the trials, the loyalty and security programs, and the numbers, always the numbers—so many investigated, so many dismissed, so many resigned under pressure—and behind the numbers the tremendous suffering inflicted, suffering only incompletely measured by imprisonment, loss of employment, divorce, and suicide. Unfortunately, this is almost the only virtue of this profoundly flawed book and one which will probably sustain few readers through its almost seven hundred pages of text and references.

To begin with, there is little that is original in Caute's book, almost all of which is drawn from secondary accounts. Although Caute perhaps deserves some credit for assembling a narrative based on many, though by no means all, of the major published accounts of the period, I nevertheless found it remarkable, even in this era of the popular history, that an author would attempt such a study without visiting the Truman Library, the Eisenhower Library, or the Library of Congress. Caute's book is also poorly organized and all too frequently inaccurate. Most recent historians will be somewhat surprised, for instance, to learn that the Council of Economic Advisors and the Atomic Energy Commission were established during World War II, that Washington's senator, Republican Harry Cain, was a Democrat, or that J. Bracken Lee, the ultraconservative former governor of Utah, was an anti-McCarthyite. Nor is the book very well written, all the more surprising since Caute is apparently a professional writer. Thus the FBI is "rewarded with real red meat" (p. 12), informers are discovered "oozing" (p. 12) into the nation's bloodstream, the Rosenbergs are accused of "a somewhat strident public attitudinizing" (p. 68), congressional committees "sink their claws" into hapless witnesses (p. 166), the Constitution is "concussed" (p. 139), while the Supreme Court's ambivalent treatment of the Fifth Amendment is but "liberal froth on the rim of the radical coffin" (p. 155).

The interpretive confusions of *The Great Fear* are even worse. Thus, although most Americans were far less concerned about domestic Communism than their political and editorial leaders (in a 1953 poll less than 1 percent of all respondents volunteered domestic Communism as a source of major concern), Caute nevertheless insists, in a typically overwritten passage, that Americans in the 1950s were "sweat drenched in fear" (p. 11). And while most recent research traces the red scare to the Cold War and to the behavior of political and economic elites, Caute instead variously attributes

the antiradical politics of the era to the Irish, the Germans, farmers, cowboys, Catholics, upwardly mobile second-generation immigrants, and, rather less specifically, "the historic fragmentation of the national culture" (p. 224). It is rather difficult to assess these and other similar assertions, however, since Caute offers no evidence to support them. Indeed, in spite of Caute's frequent references to "populist intolerance" and public hysteria, his case histories almost invariably describe the actions not of mobs, populist or otherwise, but of elite-managed institutions such as government, industry, and voluntary associations—that is, precisely those institutions whose absence he suggests produced the politics of the purge. In Caute's book, as in some earlier accounts on which he has clearly if clumsily drawn, the myth of a radical right, McCarthyite mob obscures the real political processes underlying the era.

Although Caute's analysis of causes is confused, his discussion of consequences is almost nonexistent. Thus little effort is made to trace the impact of the purge on American political and institutional life, on foreign policy, or on national thought and culture, even though it was here, and not just in the suffering of those many people whose cases Caute cites, that the most far reaching legacy of the purge is to be found. Caute was right in sensing a need for a comprehensive and contemporary account of America's second great red scare. Unhappily, *The Great Fear* does not fulfill that need.

ROBERT GRIFFITH
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HOWARD F. STEIN and ROBERT F. HILL. *The Ethnic Imperative: Examining the New White Ethnic Movement*. University Park: Pennsylvania State University Press. 1977. Pp. xii, 308. \$14.50.

I have no doubt that Howard F. Stein and Robert F. Hill labored long and arduously on this manuscript. No one could have put together so abstract, so pedantic a piece without great effort. The book does not lack merit, ingenuity, or insight; there are a number of perceptive remarks regarding the white ethnic movement and white ethnic experiences. They are, however, well hidden in a maze of arguments and hypotheses that appears to be the ultimate work in applying every available psychoanalytical theory on individual behavior to group phenomena.

The authors contend that the efforts of European immigrants and their children to attain the long-cherished American dream of success has not been realized. They hold that the era of civil rights both awakened the white ethnics to the reality of

the gap between their goals and attainments and alienated new generations of children and grandchildren from the ethnic groups and America's true ideals. Utilizing a variety of psychoanalytical concepts, Stein and Hill portray the young white ethnics as responding by the late 1960s with shame, guilt, frustration, and rage to American society. Fired up by the gains being made by non-white Americans, members of the new white ethnic generation recanted from their rebellious behavior and sought reconciliation with their parents. In so doing they initiated a movement to unite all white ethnic groups, based upon a distorted perception of true white ethnic values and historical experiences.

In their attempt to debunk this new ethnicity as a pathological phenomenon, the authors have made the "victims" (in this case white ethnics) become the causative agents of their own oppression (that is, of their own failure). So convinced are they of the contrived nature of the new ethnicity that they even speak of the "shared deception" held by involved white ethnic professionals and students (pp. 278-79). In fact, the authors argue at the very outset that a social scientist who embraces the new ethnicity is accepting "a popularly shared delusion as fact, rationalizing rather than analyzing" (p. 9). Thus, their description of the white ethnic political response to the past two decades consists of a sustained attempt to depict it as irrational and unrelated to the life style of "true" white ethnics and contrary to American patterns of assimilation.

The major difficulties with this "tendentious disquisition, selectively garnering their evidence to demonstrate a prior ideology," to quote their attack on others (p. 284), are not its psychoanalytical approach, for there is great potential in the imaginative but carefully documented use of such theories. The difficulties lie in the overambitious application of an array of them, excessive reliance on Freud, insufficient supporting data for all of these theories, the tendency to substitute assertion for evidence, and the emphasis on the many workshops the authors attended in lieu of specific reports from their fieldwork (with some few exceptions). What we have here is an enterprising exploration into psychohistory that does not really gel until the last chapter and that would have come across more effectively as a clearly stated hypothesis followed by a monograph half the size of this book.

ELLIOTT R. BARKAN
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JACK TEMPLE KIRBY. *Media-Made Dixie: The South in the American Imagination*. Baton Rouge: Louisiana State University Press. 1978. Pp. xvii, 203. \$9.95.

This is a hybrid work that obscures what it accomplishes by claiming to accomplish too much. Essentially, it is a history of ideas, attitudes, myths, and fantasies about the South and Southern people expressed since 1900 in a wide variety of forms—novels, plays, movies, documentary photographs, magazine advertisements, country music, television programs, and popular and academic histories. In brief compass it surveys changing images of two separate Souths: the "Old South" of benign masters and faithful slaves and the "New South" of moonshiners, chaingangs, downtrodden sharecroppers, and violent rednecks. Little of this is new, but it represents a useful synthesis of more specialized studies from several different fields.

The author, however, has a larger aim in mind: "critical perception is not the subject of this book," he writes. "Popular perception is" (p. 55). The operative words for him are not ideas and attitudes but media and communications. Consequently, D. W. Griffith, the filmmaker, becomes "one of the great communicators of all time" and Claude G. Bowers, the popular historian, "a flamboyantly effective communicator" (pp. 3, 28). These broad generalities are offered to buttress the author's assertion that popular imaginative and narrative works create public attitudes and, therefore, also represent the public mind. His methodological foundation is so thin, however, that whenever he refers to his thesis he awkwardly hedges it. "In so far as fiction (which sells well) may represent popular perception, it would appear . . ." (p. 164). "Never a national best seller, *The Mind of the South* has nonetheless made an enormous impact upon probably millions of people, both directly and indirectly" (p. 80).

Instructors are well aware that their students often learn from a reading assignment or lecture something quite different from what was intended. It is no safer to assume that the plots of hundreds of movies, country music songs, novels, and histories equal "popular perception." The author makes little attempt to define what he means by "popular" and gives little or no evidence of "popular" (as opposed to critical) reception of the works that he cites. This book is not about the reception or perception of images but about their creation, and this poses other methodological problems for the author. His frequent references to literature and cinema are confined to content summaries, for example, and make no reference to the role of style in shaping the "communication" of a work of art.

The book concludes with an all-too-brief assertion that a distinctive South has been almost obliterated by a dual process of cultural nationalism—a "Southernization of America" and an Americanization of Dixie. Because the plantation and redneck myths have apparently been muted in this process, it is not clear how powerful the author

perceives the media to have been in shaping social change.

ROBERT SKLAR
New York University

DANIEL A. NOVAK. *The Wheel of Servitude: Black Forced Labor after Slavery*. Lexington: University Press of Kentucky. 1978. Pp. xvii, 126. \$9.50.

Daniel A. Novak's book is the fourth study of involuntary servitude in the post-Civil War South. It is a legal study that is based on research in the literature and that produces a compendium of southern laws and cases bearing on involuntary servitude. In this respect the book is a contribution to the growing scholarship on the emergence of the labor system following the war. Yet when he attempts to go beyond the law into the historical context of sharecropping, Novak reveals a shallow understanding of the forces of post-Civil War southern agriculture. Ignoring the recent economic publications on nineteenth-century agricultural history, relying on inadequate secondary sources, and using no primary sources outside the law, his book is one-dimensional. For example, only someone who overlooked numerous relevant works would write, "One aspect of this emerging system which has not been touched on was the introduction of the invidious 'two-price' idea: the price of goods paid for in cash was far less than the price of the same goods lent on credit" (p. 30). Furthermore, to any scholar who has kept up with the expanding Reconstruction literature, it comes as no surprise that Black Codes proscribed the economic freedom of blacks, that the Freedmen's Bureau enforced labor contracts, that Radical legislatures enacted restrictive contract laws, and that the web of statutes caught many laborers and bound them to the land. Enticement statutes, emigrant-agent laws, contract-enforcement statutes, vagrancy laws, the criminal-surety system, and the convict-labor laws all eroded freedom. While Novak covers some aspects of these practices, faulty transition hinders the reader's effort to follow these laws through time, because they did not appear at once and were not enforced systematically. Rather, they reflected the many historical currents that ran through the South.

Novak proposes to study all kinds of involuntary servitude, but, instead of subsuming peonage under the general rubric of forced labor, Novak illogically reverses the categories. "The reader should be warned that, although peonage has a distinct legal meaning (i.e., debt servitude), it will generally be used here to describe any system of forced labor not under penal supervision" (p. xvii). The misuse of the crucial element of debt when writing

of peonage causes Novak problems and at one point leads him to conclude, "Further, the only sort of peonage which had been attacked was that which involved debt, 'the basal fact of peonage' in the narrow definition given to it by the Supreme Court" (p. 63). Exactly. The Supreme Court defined peonage as debt servitude, and the enforcement machinery of the Justice Department was bound by that definition as were the lower federal courts and local department officials. As deplorable as this "narrow definition" might be in Novak's eyes, the department could only attack cases of involuntary servitude involving debt because that was all that the 1867 statute and subsequent rulings allowed.

Forty-seven of the ninety-one pages of text are devoted to peonage in the twentieth century. This summary of laws and cases breaks no new ground, and some of his conclusions are contrary to the facts. "In the absence of any compelling evidence to the contrary," Novak writes, "one is led to conclude that it was the horrendous idea of white peonage which provided the final spur to federal interest in the area" (p. 47). Yet peonage prosecutions began in 1901 with the *Clyatt* case, and by 1906, when white peons entered the story, there had been intense activity in the Justice Department and the federal courts as well as wide coverage in the press. The hundreds of prosecutions certainly provide compelling evidence to the contrary. White peons, mostly immigrants, were a special case. Novak also insists that peonage "held a majority of the black population of the South in thrall" (p. 84), but he offers no evidence to support this contention. Peonage was indeed widespread, but it hardly bound a majority of blacks involuntarily.

This study, which could have made a major contribution to the study of the legal origins of involuntary servitude in the South, adds little to the existing scholarship. From a strictly legal point of view, *The Wheel of Servitude* is a workmanlike monograph; unfortunately, the book's narrow focus and numerous flaws limit its importance.

PETE DANIEL
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ROBERT M. FOGELSON. *Big-City Police*. (Urban Institute.) Cambridge, Mass.: Harvard University Press. 1977. Pp. xi, 374. \$15.00.

Until recently American police history of the twentieth century has been written primarily by non-historians. As a result, recent police history has seldom been examined as part of broader aspects of American society and has often been treated as a story of the triumph of reform and "professional-

ization." In the last few years, however, two historians have published brief interpretative histories: James F. Richardson, *Urban Police in the United States* (1974) and Samuel E. Walker, *A Critical History of Police Reform: The Emergence of Professionalism* (1977). These two works, intended in part as textbooks and based to a considerable extent on secondary materials, have laid the foundations for a solid historical understanding of policing in twentieth-century American society. Robert M. Fogelson's book, by contrast, is much longer, more encyclopedic in its coverage, and based more completely upon primary research.

There are two basic interpretative themes that dominate Fogelson's study. One theme is that police reform in twentieth-century America can be divided into two phases. The first, lasting until about 1930, was reflected in a conflict between urban political machines (with their lower-class constituencies) and upper-class or upper-middle-class progressive reformers. During this phase the military provided the model for reformers. In the second phase, lasting into the 1960s, the leadership for reform was more heterogeneous and often included criminal justice leaders such as the International Association of Chiefs of Police and the FBI. "Professionalization" rather than the military served as a model for the second phase of reform.

The second theme in the book is that police reform has been accompanied by failures and unintended consequences. At one level, failure is reflected in the fact that, even though urban police are less corrupt and lawless than in 1900, they still fall far short of reformers' goals. At another level, reform itself had unintended consequences. For instance, efforts to raise the educational levels of policemen and to base hiring and promotion on civil service examinations made recruitment of minority policemen more difficult. Also, at the turn of the century, with control by ward politicians over local police, the police provided considerable power in the hands of lower-class interest groups. But, by the 1960s when blacks had gained local political control in a number of cities, reformers had broken the control that local politicians could exercise over policing, so that black politicians found themselves without the power exercised by earlier generations of lower-class political leaders. Other factors, largely outside the control of reformers, also affected policing, including the police unionization and rising crime rates of the 1960s. The result of the failure of reform, Fogelson declares, has been a questioning of reform goals and a shattering of the coalition that supported the old reform program. Presumably, then, American policing is now entering upon a third phase of its twentieth-century history.

The book is difficult to evaluate overall. Parts of it are very good, especially the description in the first chapter of the machine-controlled, decentralized police at the turn of the century and the analysis in the sixth chapter of the professional model of policing. The book attempts a broad coverage of many big-city departments; this makes it the most comprehensive study of twentieth-century policing in America, but also makes it at times repetitious and difficult to follow. A few topics are strangely omitted. There is, for example, relatively little discussion of the impact of telephones, radio communication, patrol cars, and other technological changes upon the nature of policing. Nevertheless, the overall accomplishments of the book make it one with which criminal justice scholars and planners should be familiar.

MARK H. HALLER
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R. CARGILL HALL, *Lunar Impact: A History of Project Ranger*. (NASA History Series.) Washington, D.C.: National Aeronautics and Space Administration. 1977. Pp. xvii, 430. Paper \$6.25.

Lunar Impact is recommended for scholars of public administration as well as historians of science and technology. Following Project Ranger from its inception in 1958 as an idea of William Pickering, Director of the Jet Propulsion Laboratory (JPL), to the limited successful flights of Rangers 7, 8, and 9 in 1964-65, R. Cargill Hall's study emphasizes technical problems and organizational conflicts of the JPL with the National Aeronautics and Space Administration (NASA), the air force, the army, and industrial contractors.

Hall's volume excels in explaining the complex reasons for Ranger's frequent failures. Mission objectives and lines of authority became confused because the project originated partly as a means for gaining American leadership in the space race and partly as a scientific effort of NASA, the newly created civilian counterpart to military missile activity. The space race required attention to time deadlines, militating against reliability tests and requiring risk taking. As an early NASA project, Ranger became a football between JPL engineers trying to perfect a planetary flight machine and sky or planetary scientists hoping to have their data-collecting instruments ride aboard the spacecraft. "Pure" scientists fare poorly in Hall's account. Their incessant demands to obtain priority for instrumentation on an unproven flight vehicle would be humorous if their complaints were less pathetic. Eventually, after a series of disasters, the JPL received authority to limit the spacecraft's passenger equipment to television cameras for photographing the moon.

In addition to haste and scientific problems, Hall elucidates organizational difficulties hindering Project Ranger. Although the survey team planning Ranger's mission warned of "difficult managerial circumstances" requiring a central authority, the project's organizers ignored this advice. Four major groups sought areas of control over Ranger, each having separate identities and an existence to justify. The JPL, Ranger's contract manager, did not get the central authority it needed until 1963. For four years, JPL officials fended off demands from NASA as the contracting group, from the air force as controller of the Atlas launch vehicle and Cape Canaveral facilities, and from Wernher von Braun's army missile team at Huntsville, Alabama, regarding procurement of Agena, the second-stage launch machine. Relations between the JPL and NASA proved most treacherous, and Hall offers details about relationships between these two groups. NASA frequently supported the sky scientists' demands for experiments and attempted to oversee all decisions made at the JPL.

A major weakness of Hall's work is insufficient explanation of Atlas-Agena's unreliability as a launch vehicle. He notes that before Ranger 5's disaster Atlas "represented the principal problem" (p. 166), but causes of the problem are never examined. Lockheed Corporation was the air force contractor for Atlas, and, as a former Lockheed historian, Hall should have special insight into that problem.

Generally, Hall avoids straying far from descriptions of the JPL's technical activity and its relations with NASA. There is no satisfactory explanation of why RCA did not better handle problems with performance of the TV system or why Huntsville's responsibilities were moved to Lewis Research Center in Cleveland. After Ranger 5's failure, others in addition to JPL Project Director James Burke and Lunar Program Director Clifford Cummings should have lost their posts. We must presume Burke and Cummings took the entire responsibility for failure; others escaped blame.

Nevertheless, within the narrow focus established, Hall composes a book worth studying. The volume's appendix includes valuable data on lunar theory before 1964, technical details of Ranger's history, and a bibliography of Ranger's scientific findings.

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JOHN R. LABOVITZ. *Presidential Impeachment*. New Haven: Yale University Press. 1978. Pp. xiii, 268. \$15.00.

The historical interest of John R. Labovitz's *Presidential Impeachment* lies as much in the insights it offers into the reasoning and concerns of the Nixon impeachment inquiry staff as in his account of impeachment precedents. Labovitz's narrative of the incorporation of impeachment into the Constitution is straightforward and uninterpretive. His assessment of the impeachment of Supreme Court Justice Samuel Chase in 1805 is similar to that Raoul Berger presented in his *Impeachment: The Constitutional Problems* (1973), while his interpretation of the Andrew Johnson impeachment of 1868 is similar to the one I posited in *The Impeachment and Trial of Andrew Johnson* (1973) but with closer attention to the purely legal issues. Ultimately, Labovitz concludes, the precedents demonstrate that the critical elements in an impeachable offense are its seriousness and the intent with which it was perpetrated. As most scholars have, he finds that the gravamen of such an offense need not be criminal in the usual sense but, rather, the willful failure to execute properly the president's constitutional responsibilities. The power to impeach and try upon impeachment belongs solely to Congress, he insists, flatly contradicting Berger's notion that an appeal lies to the federal courts.

Labovitz offers the best account yet available of the legal issues involved in the Nixon impeachment investigation—noting that once again investigators rejected arguments that impeachment applies only for indictable crimes and that the seriousness of the allegations was a critical issue. Labovitz himself served on the House Judiciary Committee Impeachment Inquiry Staff's unit on constitutional and legal research. Thus, his account of the Nixon inquiry amounts to a primary source, confirming the depth of the committee's concern that its proceedings provide no grounds for charges of partisanship and demonstrating the lengths to which it went to oblige Nixon's counsel, James St. Clair. Labovitz makes manifest the committee's commitment to retaining a full and exclusive power of impeachment in the House, refusing to seek court enforcement of committee subpoenas and rejecting noncompliance based on "executive privilege." Moreover, Labovitz's discussion of the legal questions—the standard of proof for impeachment and then for conviction, the role of defense counsel, the right to plead the Fifth Amendment, and the form of the impeachment articles—is informed throughout by the staff's and committee's confrontation with those problems.

Labovitz's treatise will not permanently settle many issues in the law of presidential impeachment. The precedents are too few and the process too politically charged to permit that. But it offers the insights of a man who was intimately involved in the process, and one can only wish that people

similarly situated in the past had done the same thing.

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PHILIP B. KURLAND. *Watergate and the Constitution*. (William R. Kenan, Jr. Inaugural Lectures.) Chicago: University of Chicago Press. 1978. Pp. x, 261. \$12.50.

Future historians, charting America's intellectual currents in the last quarter of the twentieth century, may note that there was among legal scholars a return to and an insistence upon respect for the original intention of the framers together with an emphasis on a less malleable Constitution. First there was Raoul Berger with his trilogy on impeachment, executive privilege, and government by the judiciary. Then came Philip B. Kurland with his treatise on Watergate and the Constitution.

Actually, this is not a book about Watergate at all. Rather, that unhappy national seminar on the consequences of mendacity has merely served to call this essay forth and give it focus. Written with Kurland's characteristic acerbity, the book is refreshingly pessimistic.

The predominant view has been that with Richard Nixon's departure America's constitutional processes prevailed. Those who advance this thesis believe that our institutions were not at fault in Watergate, merely the men who staffed them. Kurland emphatically disagrees. With controlled exasperation and interesting scholarship, he argues that the sources of Watergate are found not in the paranoia of the King of San Clemente but in modern Americans' faith in the capacity of concentrated power to do good—quite in contrast with the Founders' knowledge of power's tendency to do evil.

Condemning the demise of the principles of federalism and the separation of powers, Kurland takes us on a tour—and a *tour de force* it is—of those “dull” subjects of constitutional arrangement that academics abandoned in their search for “relevance” in the 1960s: the delegation of legislative power, control over the appointment and (more importantly) the removal of federal officers, Congress's investigatory powers, executive privilege, impeachments, and the presidential power to pardon. His conclusion is a clarion call for a return to faith in institutions wrought by intelligence rather than in personalities molded by public relations. It is not enough that the president has been born again. The Republic must be born again.

Yet, what reforms emanated from Watergate? There was the attempt to regulate campaign prac-

tices, with an emphasis on public financing, despite the fact that “there was nothing in the Watergate record to require or even to justify public funding of national elections” (p. 187). Half of this effort was, of course, declared unconstitutional. But is it not of interest that, rather than attack the problem by focusing on Article II, Congress chose to attempt to constrict the First Amendment?

The wellsprings of Watergate were the concentration of power in the White House and the failure of Congress adequately to check the executive as intended by the framers. As for the latter, sporadic attempts at reform such as the creation of a permanent congressional legal counsel have failed, despite the compelling evidence of a need for them. As for the former, the Senate has announced itself pleased that the nation's intelligence agencies are now fully accountable to the president—as a result of an executive order of President Carter. “What is forgotten about Watergate is that it was the FBI that blew the whistle where the CIA would have been compliant. What if there had been only the CIA?” (p. 193). One of the Nixon administration's most egregious sins was the creation of a political police force. Such a force is dangerous to civil liberties when it is not subject to executive control, as it was not under Nixon. But, as Solzhenitsyn reminds us, it is even more dangerous to human rights when it is!

The problem identified by Watergate lay not with the president but with the presidency. At a more fundamental level, it lies even deeper. Watergate was directly concerned with an overreaching executive. But during the past generation the constitutional system has also been seriously imbalanced by judicial aggrandizement. An imperial judiciary is to be feared no less than an imperial presidency, and Kurland is scathing in his condemnation of courts that consistently rule in favor of their own authority to define the public interest. *United States v. Nixon* may stand for the proposition that no man is above the law, but it also declares that the judges are the law. “Sooner or later, this country must directly face the question whether it is prepared to entrust the judiciary with the mantles of Plato's guardians. The answer to that question will also determine the future of American democracy and, perhaps, even of American liberty” (p. 74).

This slim volume is a fitting beginning to the decade that will prepare for the celebration of the bicentennial of the Constitution, for Kurland leaves little doubt that it was not the Constitution we were given in 1787 that was to blame for Watergate but the perversions of that Constitution that modern America has countenanced. Ultimately, Kurland's challenge is reminiscent of Lincoln's: “The occasion is piled high with difficulty, and we

must rise with the occasion. . . . We must dis-enthral ourselves, and then we shall save our country."

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CANADA

C. P. STACEY. *A Very Double Life: The Private World of Mackenzie King*. Toronto: Macmillan of Canada. 1976. Pp. 256.

C. P. STACEY. *Mackenzie King and the Atlantic Triangle*. (1976 Joanne Goodman Lectures, University of Western Ontario.) Toronto: Macmillan of Canada. 1976. Pp. xv, 74. \$7.95.

These two monographs are Charles P. Stacey's latest on the King era. Following the pattern of his earlier works, he increasingly concentrates on William Lyon Mackenzie King himself. *Mackenzie King and the Atlantic Triangle* broadly sweeps King's attitudes on Anglo-Canadian and Canadian-American relations. Here Stacey sets forth the skeleton of his interpretation of Canadian external relations since World War I. This position should be developed in detail in the forthcoming second volume of *Canada in the Age of Conflict*, Stacey's survey of the history of Canadian external relations. In *A Very Double Life* Stacey analyzes King's private life. These studies substantially advance Stacey's position as a great, if not the greatest, authority on Mackenzie King.

Stacey's thesis in *Mackenzie King and the Atlantic Triangle* is that King was "in essence a Victorian Canadian from Ontario, whose inherited values were British, and who admired British ways, coveted British approval, and was devoted to the British connection" (p. 21). He differs with the liberal nationalist historians who dominated Canadian historiography in the later years of King's life and argued that King was the knight in shining armor, leading Canada toward greater autonomy from Britain. Stacey thus contributes to the conservative nationalist historiography expounded in recent years by William Morton, Roger Graham, Donald Creighton, and Sydney Wise. Stacey moves beyond Creighton in particular to a new appreciation of King. "I should not be altogether surprised if he [King] turned up, one of these days, as the patron saint of the new nationalism" (p. 68). One will have to wait for Stacey's defense of his thesis in *Canada in the Age of Conflict* before judging whether he makes his case. Here he outlines it well.

Although he recognizes surface contradictions, Stacey argues that the quintessential King "makes

pretty good sense politically" (p. 63). Toward the Americans, King's policy was "peace . . . friendship, and, up to the limits of political discretion, free trade" (p. 63). But the British were King's main concern. Canada's destiny lay in freedom within a "permanent imperial relationship" (p. 64). Although publicly declining to become involved in Britain's diplomatic posturing at the shallow level of such occasions as Chanak, King firmly believed that at crucial moments such as September 1939 Canada had no choice but to stand at Britain's side. (Here Stacey downplays the influence of O. D. Skelton and upgrades that of King.)

Believing King is best understood in a political framework, in lecture two Stacey concentrates upon King's early ministry and in lecture three examines the post-Bennett ministry. But both lectures rest on lecture one's treatment of King's personal Atlantic triangle. Arguing strongly that a conservative appreciation of personal roots is instrumental in determining human activity, Stacey concludes that King was a typical Canadian with a deep theoretical regard for England and an intense personal dislike of individual Englishmen (p. 19). Stacey then analyzes King's relationship with the American economic upper crust and the British political upper crust. In lecture three the King-Roosevelt-Churchill personal relationship is also examined.

While discussing King's private relationships in *King and the Atlantic Triangle* Stacey makes occasional reference to the King diary. In *A Very Double Life* Stacey relies almost exclusively upon the diary, "an extraordinary document, comparable in scope and interest in the journals kept by William Ewart Gladstone in the nineteenth century and James Boswell in the eighteenth" (p. xiii). It is very likely the single most important historical document related to Canada.

Stacey argues that King kept his public and private lives separate. Basically insecure, WLMK sought in his private life the flattery and security he could not acquire in public life. In seeking to establish contact with persons in the hereafter, such as his mother and political personages (invariably Liberals), King looked for information that supported his own position, not advice as to what action he should take.

Two other topics receive substantial attention. The more sensational is the discussion of women in King's life: his mother, prostitutes, women he considered marrying, and women, such as Joan Patteson, who were a constant element in his life. For this reviewer, the second topic, the role of Providence in King's life, is by far the more significant. Most historians have recognized that nineteenth-century Canada cannot be understood

without considering religion. A Victorian Canadian (as Stacey argues), King placed great emphasis upon the role of God in his life. He sincerely believed himself to be an instrument of God intended to advance the social conditions of his fellow human beings. King's uncritical judgment in spiritual matters leads Stacey to conclude that this consummate politician was a man of second-rate intellect.

Each of these monographs is fascinating in its own right. But while reading *A Very Double Life* there was always a sense that King's private life was related more closely to his public life than Stacey allowed. King's concern for the social welfare of prostitutes expressed in the diary is not separate from the concern for Canadians in the industrial age expressed in King's *Industry and Humanity*. A second problem arises: while *A Very Double Life* argues a successful separation of public and private worlds, *King and the Atlantic Triangle* suggests that the private person did affect the public situation. Stacey cannot have it both ways.

A Very Double Life is, on the surface, the more intriguing, but *King and the Atlantic Triangle* is, by far, the more satisfying.

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IVAN AVAKUMOVIC. *Socialism in Canada: A Study of the CCF-NDP in Federal and Provincial Politics*. Toronto: McClelland and Stewart. 1978. Pp. ix, 316. Paper \$6.95.

Although more has been written about the CCF-NDP, Canada's social democratic movement, than any other party, a full-scale history has been lacking. After Ivan Avakumovic's book it is still lacking. This is an altogether disappointing effort, although its shortcomings should not have been surprising after a reading of Avakumovic's other recent book, *The Communist Party in Canada* (1975).

Socialism in Canada is a family history of social democracy. It periodically engages in mildly disapproving asides, but for the most part the book is written from the official viewpoint and is sanguine in the extreme about the future of the NDP. The family history tone is carried through in the presentation. The study is chatty and rambling, almost conversational. It remains on a general level, providing little in the way of detail, dates, names, or other specifics. Fairly typical is an account of what NDP leaders do (pp. 218-19). After a banal listing of speechmaking, writing letters, and so on, the subject is summed up: "Any NDP leader who took his or her duties seriously was kept very busy. Rest and relaxation were at a premium." That will certainly show up in many political science foot-

notes. Avakumovic himself, however, is not a great believer in footnotes. Although the acknowledgements list many archives and libraries, the footnotes are sparse and refer primarily to obvious published sources. This does not help the problem of vagueness that permeates the book.

Avakumovic accepts the official line of the CCF-NDP leadership that electoralism was the most appropriate strategy and that moderating socialist ideology was necessary for the success of that strategy. This is an arguable position. Avakumovic often proceeds, however, to develop it by unfair attacks on its opponents. For example, he lauds CCF member of parliament, A. A. Heaps, for his militancy on World War II, as contrasted to the pacifist wing of the party: "In retrospect it is obvious that this English-born ex-worker had a much better grasp of what Nazism represented and how it should be countered than many CCF intellectuals who had studied at the best Canadian and British universities" (pp. 83-84). This argument by *non sequitur* is balanced by outright smears. He describes the NDP refusal to endorse abortion on demand: "This disappointed spokeswomen of the women's liberation movement who were, or had formerly been, associated with Trotskyist organizations" (p. 219). Avakumovic gives no indication what this "movement" was, how many of its leaders might have been Trotskyists, Liberals, Anglicans, or nonaffiliated. Such *ex cathedra* judgments make the book unreliable.

The treatment is uneven, with relatively little attention to provincial developments, where, after all, the CCF-NDP has scored its only electoral triumphs—with provincial victories in Saskatchewan, Manitoba, and British Columbia. Discussion of issues is erratic and incomplete, with the Quebec question especially lacking coherent treatment. Avakumovic's rambling, conversational style is no substitute for a well-organized and critical analysis.

The history of the CCF-NDP can be pieced together more profitably from other sources. Those especially interested have a useful general analysis now available in Norman Penner's *Canadian Left* (1977). It is more worthy of time and expense than Avakumovic's fatally flawed effort.

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Dalhousie University

LATIN AMERICA

KENNETH DUNCAN and IAN RUTLEDGE, editors. *Land and Labour in Latin America: Essays on the Development of Agrarian Capitalism in the Nineteenth and Twentieth*

Centuries. With the collaboration of COLIN HARDING. (Cambridge Latin American Studies, number 26.) New York: Cambridge University Press. 1977. Pp. x, 535. \$29.95.

These essays deal with the development of capitalist agriculture, focusing mostly on the transformation engendered by the enlargement of European and U.S. demand from the mid-nineteenth century to 1914. One gains from them a strong sense of how precarious and uneven the process was, and how incomplete it remains in some regions even to the present. Indeed, the special virtue of these essays is that they reveal the complex reality behind the abstract concept of transformation. Most of the papers are based on extensive archival research, including *hacienda* records. Since the authors have not asked the same questions of these materials and have approached them with different assumptions, however, their contributions do not bring us any closer to a theoretical resolution of the bewildering variety of interactions between landowners and peasants that characterized the agrarian transformation.

In all of the cases studied—episodes or regions in Mexico, Brazil, Argentina, Chile, Peru, Colombia, Costa Rica, and Trinidad—a tendency on the part of landowners to relax their forcible monopolization of resources and labor and to rely more on the mechanisms of the market is certainly discernible. This process seems to have depended on the depth and strength of export demand and was limited by the resistance of workers, who seem to have sought most often the restoration of pre-existing relations. The landowners also slowed down this process rather than unnecessarily risk privileges they wished to maintain. There were, however, many local variations. In Costa Rica peasants did not lose their titles, continued to produce for the market, and limited the bourgeoisie to credit and processing functions. In Colombia they demanded higher wages, not a restoration of traditional rights. Although in most of the areas studied peasant incomes fell as capitalist agriculture was introduced, in São Paulo and the wheat-growing areas of Argentina they rose. The abolition of slavery presented an almost unmanageable threat to the profits and land titles of landowners in Colombia and Trinidad, but in Brazil their position was, if anything, strengthened.

Such contrasts may be simple errors of data collection or interpretation, or they may indicate the need to consider variables other than access to foreign markets. The editors, in their introduction, discuss earlier typologies of peasants and regional culture complexes, but finally conclude that the time has come to go beyond them. Magnus Mörner, drawing upon a broad familiarity with the literature, recalls in his summarizing chapter those

discussions that centered on the nature of the product exploited, but he then distinguishes other variables, principally the size of the labor force relative to demand. The editors also devise a typology based mainly on this factor, but it is too heterogeneous.

The authors are much richer in their suggestions, although only two or three of them link theory to observation in any straightforward manner. One who does is Henri Favre, whose analysis is brilliantly historical. He shows that, for highland communal Indians in Peru who experienced population pressure, seasonal wage labor on coastal plantations provided a seemingly advantageous alternative to intercommunity cooperation, enabling them to carry out subdivisions of villages beyond the limit of ecological rationality. The capitalist plantation thus guaranteed the survival of communal society, but at a level so fragmented and competitive that it could not challenge the plantation's dominance. Favre makes no reference to Rafael Baraona's idea of antagonistic realms of landowner and peasant enterprise, although Juan Martínez Alier and Cristobal Kay both make effective use of it. The concept has the advantage of restoring conflict to central view and of taking the peasants as well as the landowners as its subject.

A variable that may partly determine the direction of such conflicts is whether or not the product is a novelty. If the product remains the same (for example, sugar in Pernambuco) and the only change is an increase in capital-intensiveness, the struggle may be contained within the bourgeoisie, as certain of its members are eliminated. Interclass hostility seems to be intensified, however, when the capitalist expands onto the lands of the same peasants he must then recruit for wages. But it is reduced when he expands into a frontier whose scattered inhabitants he can replace with seasonal workers from other regions. In the former case the conflict seems to center on restoration of rights, in the latter on compliance with contracts. Frontiers may therefore be simultaneously less capital-intensive and more capitalist in labor relations. Whatever the reader may conclude about these essays, he or she may well wish there had been more conflict among the contributors before publication, so that both they and their public could have drawn firmer conclusions from their laboriously collected data.

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HOBART A. SPALDING, JR. *Organized Labor in Latin America: Historical Case Studies of Workers in Dependent Societies*. New York: New York University Press. 1977. Pp. xv, 297. Cloth \$15.00; paper, Harper and Row, N.Y. \$5.95.

Scholars of dependency theory have rarely written on working-class history, perhaps because most of them stress the role played by economic relations between the capitalist center and the underdeveloped periphery rather than the traditionalist Marxist view of history as a process rising from the system of production and attendant class conflict. Hobart A. Spalding, Jr. has ably corrected this deficiency, writing a clear, well-reasoned "dependista" history of Latin American labor. It is also, for now, the most reliable general survey of the field, supplementing and in many cases replacing the earlier studies by Carlos Rama (1959), Moisés Poblete Troncoso and Ben G. Burnett (1960), Robert Alexander (1965), and Victor Alba (1968).

Much of the Latin American labor history written in the 1960s came from liberal historians operating under the premise that the Latin American workers' only hope for material progress lay with democratic reform parties pursuing developmentalist policies. Given the resurgence, however, of conservative military regimes after the mid-1960s, and faced with mounting evidence that the impressive record of post-World War II economic growth has been accompanied by relative and in some cases absolute decline in the well-being of Latin American working people, many labor historians now hold out little hope for peaceful social change. Spalding's book provides the historical and conceptual framework for this emerging radical critique of Latin American labor history.

The first third of the book deals with labor from the nineteenth century through the Depression. The remainder of the book is divided into four topical chapters: Mexico, 1910-70; labor and populism in Argentina, 1943-55, and Brazil, 1930-64; labor and revolution in Bolivia, 1932-71, and Cuba, 1935-61; and "the imperialist thrust," an analysis of U.S. government and U.S. organized labor's relations with the Latin American labor movements. There is no bibliography, unfortunately, but the author provides a useful index.

Spalding contends that international capitalism shaped Latin American economic development by limiting "the range of possibilities" open to domestic elites, which, in turn, significantly affected the relationship between the ruling classes and labor. While acknowledging the influence of varying local and national conditions on the historical course of organized labor, Spalding argues that "the international dimension weighs heaviest" (p. 282). Unionization has failed to improve the material and political position of labor because of repression of its militants, cooptation of labor leadership and certain elite sectors of the working classes, intervention by imperialist agencies, official U.S. policy, and endemic working-class disunity.

The author has marshaled an impressive array

of facts drawn from primary sources and secondary literature, utilizing most of the recent scholarship. There are, however, a few weaknesses in argument and interpretation. European ideologies, on the one hand, and miserable working conditions, on the other, are too often put forth as the most important factors spurring workers to action, with little regard for such factors as technology, production patterns, or the roles played by the differing social, regional, and industrial origins of the workers themselves.

More serious is the author's contention that working-class consciousness is a function of long-term struggle against class adversaries. Hence, he concentrates on the details of labor organization and conflict, omitting any exploration of working-class culture and social experiences, which play important roles in formulating worker perceptions of class interest. Also debatable is Spalding's decision to conclude his coverage of Cuban labor in 1961, contending that Cuban workers "increasingly made basic decisions affecting their lives and played a growing role in deciding the future course of Cuba's transition to socialism" (p. 252). Many readers who might be prepared to accept the premise that the Cuban working class is better off under socialism will wonder at the claim that Cuban labor's integration into the revolutionary power structure has been without problems worth discussing in a book on Latin American labor.

Nonetheless, the work is sound and important. Spalding's earlier chapters are useful, his coverage of Mexican labor the best available in English, and his analysis of labor under populist regimes valuable. Particularly significant are Spalding's controversial conclusions that U.S. support of reformist regimes, such as the MNR in Bolivia, and of "democratic" trade unionism may in fact undermine any chance for social justice in Latin America. Indeed, he points out that, ironically, U.S. labor policy in the hemisphere has been most successful in those nations whose governments are tied to conservative political parties.

In summary, Spalding has written a useful and important book and has posed, far better than most North American labor historians, a troubling dilemma—that the history of Latin American working people offers little solace to those who place abiding faith in democratic institutions and in peaceful solutions to complex social problems.

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JOHN K. CHANCE. *Race and Class in Colonial Oaxaca*. Stanford: Stanford University Press. 1978. Pp. xvi, 250. \$14.00.

There have been few specialized studies of inter-

ethnic relations in colonial Spanish America. Consequently, the decision by John K. Chance to explore the complex and changing nature of the interaction among Indians, Spaniards, Africans, and their progeny in a colonial city should be welcomed. Chance, an anthropologist, has written a fine study of the city of Oaxaca (formerly Antequera) in southern Mexico that considerably advances our understanding of the evolution of an urban social structure during the colonial period.

The choice of an urban area as the locus for such a study is judicious since cities were the only places likely to have representatives of the various racial and ethnic groups. In addition, as Chance notes, Oaxaca was similar in its racial composition to most colonial cities in Mesoamerica. Founded in 1521, Oaxaca became the third most populous city in colonial Mexico, exceeded only by Mexico City and Puebla. The author's principal objective is to analyze, in Oaxaca, the growth and operation of the *sistema de castas*, which he defines as "a cognitive and legal system of ranked socioracial statuses created by Spanish law and the colonial elite in response to the growth of the racially mixed population in the colonies" (p. viii).

The work is based on secondary literature as well as on an impressive amount of archival information gathered in Oaxaca, Mexico City, and Seville. Although the author suggests that he examined "all the relevant documentation pertaining to all segments of the urban population during the entire colonial period" (p. viii), the reader must consider this assertion with caution. Chance's analysis of occupational and marriage patterns and other data demonstrates that by the eighteenth century economic factors were likely to be more important than racial ones in determining social status in Oaxaca. He finds, not surprisingly, evidence of a good deal of racial mixture and of racial passing. Marriage alliances between the mixed groups (*castas*) and the Spaniards led to much upward mobility in status for the nonwhites. The *sistema de castas* is shown to have been less rigid than hitherto believed. In fact, the signs of its demise were present in Oaxaca as early as the seventeenth century.

Chance finds that the most enduring racial barriers were directed against the peoples of African ancestry, although by the eighteenth century some mulattoes could find marriage partners in the white group. The reader may find it difficult, however, to reconcile the larger society's unfriendly attitude toward blacks with the author's acceptance of a model that ranks them above Indians in the social hierarchy. One may also question the basis for Chance's conclusion that there was an absence of racial consciousness among the mixed groups and that the term "ethnic group" is con-

sequently inappropriate when used in discussions about them. Since this assessment relates essentially to how people define themselves, as opposed to how they are defined by others, it must be supported by a much broader variety of evidence than is presented in this book. Nevertheless, this is a thoughtful study that is often quite persuasive in its arguments. Other scholars will most surely be stimulated to test the applicability of its findings to other parts of the Spanish empire.

COLIN PALMER
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CHRISTON I. ARCHER. *The Army in Bourbon Mexico, 1760-1810*. Albuquerque: University of New Mexico Press. 1978. Pp. xv, 366. \$15.00.

The author of this carefully presented and thoroughly documented study of the army in Mexico from 1760 to 1810 concludes that the post-independence experience of José María Mora and other historians of the nineteenth and twentieth centuries colored their views of the late colonial period, especially with regard to the martial spirit and the creation of an autonomous praetorian tradition. In the first four chapters Christon I. Archer analyzes (1) the various proposals offered by the military and sometimes approved by the Spanish government for the formation of an army of New Spain; (2) the problems entailed in the defense of "strategic Veracruz"; (3) the struggle between the residents of the port and the viceroys; and (4) Spain's perception of the danger that a separatist government might result from the creation of a strong army in its colony of New Spain. These chapters are followed by a perceptive description of the army bureaucracy and the Bourbon administration; the army and the *cabildos*; the merchants and the military; the officer corps; manpower; discipline, punishment, and conditions of service; and finally the military developments from 1808 to 1810 when New Spain was "cast adrift" by the Napoleonic invasion of Spain.

Archer challenges many of the myths about the army in New Spain: its autonomy, its size, its *fuero*, the struggle between European Spaniards and *criollos* for army commissions, and so on. He demonstrates that the supposed autonomy of the army in New Spain was short-lived, if it ever existed at all. In fact both the viceroy and the *audiencia* exerted control over the military in New Spain. Furthermore, the size of the army—regular and provincial forces as well as the militia—was quite different on paper than it was in fact. And, generally speaking, not even the dregs of European or *criollo* society wanted to serve in the army. The military *fuero* was constantly challenged by the

other corporate bodies; it applied primarily to commissioned officers and only to the militia forces when they were on active duty. While some *criollos* were briefly attracted by army commissions offered for sale in the 1780s, they soon became disillusioned because military duty interfered with their economic interests. Even European Spaniards disliked the demands of military service. Since many of the European officers in New Spain were not from the higher levels of Spanish society, they failed to set an example for the colonials whom they were supposed to inspire with patriotism and discipline. Notwithstanding a few exceptions, such as Félix Calleja and his forces in San Luis Potosí, this description applies to most of the military in New Spain from 1760 to 1810.

This excellent work is a must for everyone interested in late eighteenth-century Spanish colonial history in the New World, especially that of Mexico. It also highlights the need for a similar study covering the period 1810 to 1854.

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LEON G. CAMPBELL. *The Military and Society in Colonial Peru, 1750-1810*. (Memoirs of the American Philosophical Society, number 123.) Philadelphia: The Society 1978. Pp. xviii, 254. \$10.00.

Leon G. Campbell's admirable study of militarization in late colonial Peru comes on the heels of Christon I. Archer's similar treatment of military affairs in Mexico, *The Army in Bourbon Mexico, 1760-1810* (1978), and gives us a preview of Allan J. Kuethe's soon to be published examination of the topic for the Viceroyalty of New Granada. All of these books were inspired by Lyle N. McAlister's pioneering studies on the military in New Spain and all have advanced our understanding of the roots of militarism in Latin American politics and society. Campbell's purpose is to examine as closely as possible the degree of militarization that occurred in Peru prior to Independence. Certain clear and provocative interpretations are presented in some areas while in others the lines of historical development are weakly drawn, vague, and sometimes inconsistent. But anyone grappling with the Peruvian reality—especially in attempting to come to grips with what motivated Peru's vacillating Creoles in the late colonial and Independence periods—will find the subject matter itself difficult, often contradictory, and not very easily categorized.

The Seven Years' War triggered important reforms within the Bourbon Spanish Empire, not the least of which were military reforms aimed at bol-

stering defenses in the wake of English threats. Noting that the "army of Peru at mid-century was merely a token force" (p. 19), the author then describes the expansion and creation of many regular and militia units in the period 1763-76. Although he denies that this reorganization "militarized" Peru, Campbell nonetheless contends that "an army had been created in Peru by 1776" (p. 67). This army was put to a severe test by massive rebellions in the highlands in the 1780s—especially the one inspired and led by the latter-day Inca, Tupac Amaru II. The army's performance was compromised by mixed loyalties during this period, and from the 1780s until the Napoleonic Wars the monarchy "sought to hispanicize the army . . . to assure its continued loyalty" (p. 177). Its performance during the Wars of Independence is not presented clearly, probably because of the difficulty in sorting out true loyalties and commitments from opportunism and self-preservation in that period. Campbell does stress that the army emerged intact from the Wars of Independence—frayed about the edges to be sure—as a powerful interest group for the preservation of racial (white) and class (Creole elite) privilege.

The crux of the problem is *how* militarized Peruvian life had become by Independence. Traditional views have pictured Simón Bolívar as the *caudillo* prototype and the renewed emphasis on the military in the late Bourbon period (expanded militias, increasing expenditures on defenses, attraction of the Creoles to the praetorian virtues of military life, and so on) as the historical roots of modern militarism in Latin America. It is an important question, for the military in modern Latin America does play an active, dynamic role in politics and society that often belies the lip service given to civilian, constitutional rule.

Campbell's answer is that, unlike in New Spain, militarization did not advance very far in Peru, although Archer's study has modified some of McAlister's earlier conclusions on the degree of militarization that occurred in New Spain. A chapter on expanded military privileges in Peru, for example, indicates that few inroads were made by the military establishment on the rights and privileges (the *fueros*) of mercantile and ecclesiastical interests that possessed strong corporate identities. Furthermore, jurisdictional disputes between civil and military courts usually favored traditional civil authority. The failure of the militia to mature in the wake of Peninsular suspicions after the Tupac Amaru rebellion is further underscored by Campbell as an indicator of minimal militarization.

So we are left at the end of this important book with sound conclusions based on archival evidence (most of the author's sources are primary and

archival), but with a certain nagging dissatisfaction. If militarism did not develop fully prior to and during the Wars of Independence, when did it develop? The posturing *caudillos* of the nineteenth century capitalized on a national willingness to be led by military chieftains, and this tendency certainly had strong roots in the long period of the Wars of Independence. These struggles were marked by intermittent military campaigns that dragged millions of souls into the twilight zone of war where civil authority and tradition slowly eroded. It was patently difficult to recover a strong civil tradition after prolonged warfare. When the political system itself was altered radically, the ad hoc arrangements of war proved more enduring than the new and often experimental forms (such as constitutional republics), which commanded little loyalty.

Campbell's book is basically a well-researched piece of work that fulfills a major responsibility of the historian: to search for hard answers in the rockbed of history, the primary sources. His thesis will certainly provoke conflicting reactions, for the evidence can be read in different ways. Those interested in exploring that pervasive modern phenomenon—militarism, or why soldiers run so many Latin American countries—will benefit from Campbell's balanced and intelligent presentation of how it probably began in late colonial Peru.

LAWRENCE A. CLAYTON
University of Alabama

DAVID P. WERLICH. *Peru: A Short History*. Carbondale: Southern Illinois University Press. 1978. Pp. xii, 434. \$17.50.

Until the 1960s there was no general history of Peru in English. Then three were published in rapid succession: R. J. Owens's *Peru* in 1963, David A. Robinson's *Peru in Four Dimensions* in 1964, and Fredrick B. Pike's masterful *The Modern History of Peru* in 1967. In 1976, Henry E. Dobyns and Paul L. Doughty contributed the very useful *Peru: A Cultural History*. All of these works have their strengths and weaknesses, but taken together their existence would seem to preclude the need for another "history of Peru" unless that new volume made important new contributions in either research or interpretation.

Unfortunately, David P. Werlich's book does neither. The volume contributes no new research findings and offers no new interpretations of Peruvian history. In fact, it borrows quite heavily from several published sources. Moreover, it completely lacks either a conceptual framework or a set of themes that would tie the various periods and events together in order to achieve a modicum of historical unity.

The book begins with a rather traditional introduction to the land and people of Peru and then attempts to treat the pre-Columbian past (including the Inca empire) and the entire Spanish colonial period in thirty pages. This discussion fails to develop any themes for subsequent treatment, a defect which mars the next three chapters as well (covering the period 1808–1914). Whereas Pike's treatment of this later period is rich both in detail and interpretation, Werlich presents a rather pedantic compendium of disconnected data and fails to explain the importance of Hispanic colonial institutions, Spanish concepts of the origins of wealth, Catholic Church control of education, or military perceptions of their societal role. Without such explanations, the actions of Ramón Castilla, José Balta, Manuel Pardo, and the principal politicians of the twentieth century are not easily understood.

The remainder of the book treats the period 1914–76 in six chapters, but again Werlich's analysis of such major figures as Augusto B. Leguía, José Carlos Mariátegui, Víctor Raúl Haya de la Torre, and Fernando Belaúnde Terry is both unsophisticated and unimaginative. Particularly disappointing is the analysis of the "new militarism" after 1968. This section fails to place the phenomenon in its historical perspective and to relate it to the rest of Latin America. On this topic Werlich falls below the mark set by several previously published works on the Peruvian military's revolution, particularly Abraham F. Lowenthal's edited volume *The Peruvian Experiment* (1975).

While often attaining a level of journeyman competence, this work is not a valuable contribution to the historical literature on Peru. It is neither a useful textbook nor a popular history with great appeal. All of these audiences must continue to rely upon Owens, Robinson, Pike, and Dobyns and Doughty.

THOMAS M. DAVIES, JR.
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MARTIN T. KATZMAN. *Cities and Frontiers in Brazil: Regional Dimensions of Economic Development*. Cambridge: Harvard University Press. 1977. Pp. 255. \$16.50.

This volume of case studies by an economic geographer is a stimulating contribution to our understanding of modern Brazil as a continental nation in the throes of development. Have the industrialized regions exploited the backward regions? Why does urbanization continue at such a frantic rate in the face of unemployment and underemployment in the major cities? What is the real potential for population transfers to the Amazon basin? To

what extent have planned cities such as Brasília created jobs? To what extent does the "growth pole" approach explain the patterns of Brazilian settlement and development? Is neoclassical equilibrium analysis a more reliable guide?

Martin T. Katzman deals with these difficult questions in a sure-handed manner. On the whole he comes down in favor of neoclassical explanations, although he carefully considers the explanatory power of the hypotheses identified with François Perroux. He finds, for example, that since the 1930s the impoverished northeast has experienced a net inflow of tax funds, while the dynamic state of São Paulo suffered a net outflow in fiscal terms. The conclusion is that the growing regional inequalities in Brazil cannot be explained by the center's exploitation of the periphery. Yet Katzman has not delivered the last word on this important controversy, since his consideration of the nonfiscal dimension is minimal.

Katzman suggests at one point that structuralism—so popular with Latin American economists—is merely a "Latin American elaboration of the polarization paradigm" (p. 134), an intriguing thought. More to the point, he argues, is Brazil's persistent subsidization of capital, when the huge surplus of labor would seem to suggest the need for a bias in its favor. The implication is that wage rates have been too high, especially at the margin. In Katzman's view the increasing social security benefits have only reinforced this distortion. Those who denounce Brazilian wage policy since 1964 as too antilabor must therefore look to their statistics. The question is total employment, argues the author, not wage rates.

Katzman's overall theme is "an understanding of the impact of urbanization on rural development in Brazil" (p. 8). He frequently tests for the presence of the "labor safety valve," a topic of interest to students of comparative frontier experience. Brazil has been one of the few countries in recent decades able to bring into cultivation previously undeveloped land. This safety valve is fast disappearing, and Brazil's ability to supplement it with genuine land reform is very much in question.

However much readers may differ with Katzman's conclusions, his analysis will force them to rethink some of the most basic questions about contemporary Brazilian history.

THOMAS E. SKIDMORE
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MARY NOEL MENEZES. *British Policy towards the Amerindians in British Guiana, 1803-1873*. New York: Oxford University Press. 1977. Pp. xiii, 326. \$18.95.

This is a history of governmental policy toward the Amerindians of British Guiana (now Guyana)

from the inception of British rule in 1803 until 1873. As background Mary Noel Menezes describes the long Dutch occupation and recounts other European contacts since 1500, when Vicente Yáñez Pinzón first heard the tales of El Dorado.

The author, personally acquainted with Guyana, undertook research in Public Record Office documents, mission archives, and other British and Guyanese depositories. Her extensive bibliography also includes secondary materials treating in general the Spanish, Dutch, and English colonial experiences.

The book is organized into chapters that present various aspects of Indian relations—Indian ethnology, gifts and subsidies, legal jurisdiction over the aborigines, and the roles of government officials and missionaries. The work concludes with a balance sheet on British policy. This format, unfortunately, prevents any smooth narrative flow and leads in places to confusing repetition. Thus, the reader is led twice through the coming of the first Europeans and three times through the abolition of black slavery.

The Indians of Guyana—the Caribs and their affiliates, the Arawaks, the Wapisana, and the Warrau—appear here as Europeans have traditionally described them: elusive, nomadic, and "heathen." And the story is the same here as elsewhere in the hemisphere: the demographic decline of the Indians and their failure to be incorporated into a growing plantation economy led to the wholesale importation of African slaves.

The Dutch began the practice of regularly giving the aborigines gifts of cloth, mirrors, and other articles. In return, the Indians hunted runaway blacks and usually maintained the peace. British governors continued this policy, but not consistently, for they always had to deal with a reluctant Colonial Office and a planter-dominated legislature in the colony. After the abolition of slavery in 1833, the local powers could see little use for the Indians. Government officials, whose task it supposedly was to protect the Indians instead sometimes exploited and degraded them; in 1873 the last of these was replaced by a lone revenue collector. The missionaries in whose care the Indians were left, although often dedicated, helped to supplant Indian cultures by their efforts to "reclaim the untutored." The inevitable back country development led to the alienation of native lands, and the Indians became pawns in border struggles with Venezuela and Brazil. The author concludes that, for the British Guiana Indians, "there was no consistent policy."

This work is valuable since it illuminates one colonial experience and attempts to compare the impact of competing colonial systems upon native Americans. Menezes demonstrates that in Guyana as elsewhere the Indians have been acted upon

rather than being prime movers in their own destiny throughout the colonial era.

EUGENE LYON
Seville, Spain

FEDERICO G. GIL *et al.*, editors. *Chile, 1970-1973: Lecciones de una experiencia*. (Colección de Ciencias Sociales.) Madrid: Editorial Tecnos. 1977. Pp. 470.

The Institute of Latin American Studies at the University of North Carolina, Chapel Hill, is to be commended for sponsoring a seminar in the spring of 1975 which brought together participants in and observers of recent Chilean affairs. This book is the result of the meeting and is more than just another polemical addition to the current inflated body of literature on Salvador Allende's regime and the military coup which brought it to an end.

The seminar concentrated on four major themes: a description of events in Chile between 1970 and 1973, the causes of the coup, the character of the military regime that replaced the civilian government, and, perhaps most intriguing, a comparison of the Chilean experience with other situations such as Spain during the Second Republic. Some important themes not dealt with in detail, either because of a lack of time or information, include analysis of the present Chilean military, the role of the CIA and the United States government before and during the coup, and Allende's role in the chaos preceding the coup.

Nevertheless, the presentations help clarify some of the political, social, and economic issues which came to a head during those tortuous years. Since the meeting allowed discussion, debate, and rebuttal, a more balanced view than those offered elsewhere sometimes resulted. In one such example a number of participants closely examined the activities of the PDC (Christian Democratic Party) during the years 1970-73. Rodomiro Tomic, Christian Democratic candidate for the presidency in 1970, bitterly contested remarks made by Jorge Tapia Videla, former professor at the University of Chile and the Catholic University. Tomic objected to the effort made to divide the Chilean political scene neatly into Right, Center, and Left, with the Christian Democrats occupying the centrist position. He insisted there was a strong leftist wing within the Christian Democratic Party and noted that during the election campaign he had been branded by the Right as being the same ideologically as Allende. Tomic also rejected the contention that the leftist element within the PDC had believed a military coup inevitable. He agreed that Allende and his government had lost control of the general situation and of the coalition UP (Popular Unity) that had brought him to power but argued

that alternatives to a military coup had remained open. Allende could have stepped down and called a plebiscite, as he was apparently considering, or both Allende and congress could have resigned after scheduling new elections.

Finally, Tomic vehemently denied that it was the PDC which had polarized Chilean politics between 1970 and 1973. He stated that Allende's coalition had refused to cooperate with the PDC, in spite of the latter's willingness. He flatly stated that Popular Unity had intended to divide and destroy the Christian Democrats, thus ruling out all cooperative efforts.

Julio Silva Solar, former deputy in congress, denied Tomic's accusations and stated that Popular Unity had in fact had no policy regarding the Christian Democrats. Furthermore, he argued that to characterize the PDC as a party with leftist trends was to misrepresent reality; the party had drifted to the Right during 1970-73 and opposed a democratic leftist government. The PDC, according to Silva Solar, was purely a party of the bourgeoisie, with popular elements linked to that class. Its response to socialist reforms was predictable even though its leaders understood that a military coup and fascism might result from their policy. While critical of the PDC, Silva Solar saw that the UP had also faltered: the coalition was always seriously weakened by internal divisions, the economy was mismanaged, and the state failed to keep order.

This type of exchange makes the book lively and provocative. Since many of the seminar participants were directly involved in Chilean politics, their observations are particularly welcome. Unfortunately, representatives of more conservative opinion were not included in the conference. But, as the editors point out, if all shades of opinion had been voiced, the chances of a "dialogue would have been remote." Still, the conference was an important attempt to arrive at a fair and balanced view of what went wrong in Chile, and the report of its proceedings is a noteworthy addition to the literature.

TERENCE S. TARR
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PAUL E. SIGMUND. *The Overthrow of Allende and the Politics of Chile, 1964-1976*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1977. Pp. xiii, 326. Cloth \$14.95, paper \$5.50.

This study by a well-known specialist in Latin American politics is a welcome addition to the vast bibliography on the Popular Unity government in Chile. The book is all the more welcome because it is a balanced account that avoids strident pole-

mical pronouncements and because it compares and contrasts the Allende period with the preceding administration of the Christian Democratic president Eduardo Frei.

The main part is a description and analysis of Chilean political developments from 1964 until 1976, with special attention paid to the political parties and groups, their internal conflicts, and their mutual relations. All of the elections that took place between 1964 and 1973 are treated in great detail and compared with each other (a summary table with data on electoral results would have been helpful). Paul E. Sigmund also devotes considerable space to economic factors. He surveys and evaluates Frei's agrarian reform and the "Chileanization" of copper companies, and then traces the course of economic developments during the Allende period, from the initial success in 1971 to the breakdown before the coup in September 1973. Relations between Chile and the United States constitute the third theme discussed throughout the book. The author uses information made public in recent years in order to document U.S. direct involvement in Chilean internal affairs, which culminated in the "destabilization" policies supervised by Henry Kissinger.

Dissociating himself from "mythmakers of left and right," Sigmund characterizes Allende as a "skilled parliamentary politician" with a strong commitment to social justice and with romantic admiration for revolutionaries like Castro and Guevara (p. xiii). His government, according to Sigmund, was overthrown mainly because of internal problems, of which the most important were the following: first, the general difficulty of bringing about fundamental structural changes within a country that is politically very diversified and economically weak and vulnerable; second, the erroneous economic policies of the Popular Unity government; and third, the political misjudgment of Allende who, instead of increasing his support among the population, antagonized large segments of the initially sympathetic middle class. The author is convinced that the external pressure from the United States played a contributing, but not the decisive, role.

The first and last chapters also contain some considerations on the broader questions posed by the Chilean experience. The author concludes that recent Chilean history demonstrates not the incompatibility of equity and liberty, but only the difficulty in achieving these two aims.

Sigmund's book is a very good scholarly study that should be read by everyone interested in the Allende period, but it has one shortcoming. Despite the author's statement of his assumption about the direct influence of ideas on political conduct and decision making, he discusses almost

exclusively political events and pays relatively little attention to the analysis of ideas. For instance, he treats "Marxism" as a clearly defined ideological system and does not try to identify the concrete sources of the Marxist conviction of leading Chilean leftists. Similarly, he presents only a very cursory overview of the ideology of the Junta (over half of the section entitled "The Junta Ideology" deals with the corruption of the Popular Unity government members). It would have been interesting also to devote a few paragraphs to the book on geopolitics that Augusto Pinochet published in 1968.

JÍŘINA RYBÁČEK-MLÝNKOVÁ
Princeton University

KYLE STEENLAND. *Agrarian Reform under Allende: Peasant Revolt in the South*. Albuquerque: University of New Mexico Press. 1978. Pp. xix, 241. \$5.95.

Kyle Steenland's book is divided into two parts. Part one contains two chapters, the first describing the rural strategy under the Popular Unity government (1970-73) and the other examining the class relations in Chilean agriculture from 1540 to 1930. The latter chapter tends to be superficial; an analysis of the period 1930 to 1970 would have been more relevant. Some historical background on the colonization and development of the frontier region where Lautaro—the area of Steenland's research—is located is also lacking. Nevertheless, in chapter two important issues are discussed, such as the question of feudalism and capitalism. Yet the author's evidence for contending that feudal oppression, operating through extra-economic mechanisms, predominated in the countryside until the 1930s is not convincing. The existence of abundant rural surplus labor eager to obtain tenancy on the large landed estates and the occurrence of large-scale migrations point to the mobility of rural labor and do not support a feudal characterization. Unfortunately, the author did not consult a key book by Arnold Bauer, who analyzes precisely this period of Chilean rural society from the Spanish conquest to 1930.

Part two has ten chapters which are concerned with a detailed chronological analysis of peasant mobilization and agrarian reform in Lautaro during the Allende administration. This is the most stimulating and original part of the book. Most of the material was collected first hand in Lautaro, Cautin Province, where the author lived from 1972 to 1973. Anyone wishing to gain an insight into the most crucial rural transformations in Chile will be well advised to consult this book, particularly since many of the issues Steenland analyzes in detail are relevant to the whole of rural Chile. The large-

scale seizure of farms by peasants was first carried out in Cautín Province and the class struggle acquired particular intensity in this region. This is partly explained by a distinguishing characteristic of this region: seventy-five percent of the rural population are Mapuches, who had long-standing grievances with landowners over appropriation of their lands. The narrative analyzes not only the great achievements of Popular Unity's agrarian reform but the difficulties as well. Familiar problems emerge: the divisions between the various left-wing parties, the conflicts between the peasants and the agrarian bureaucracy, the tendency for the beneficiaries to concentrate their effort on individual holdings and neglect collective produc-

tion, the question of shortages and the black market, and the existence of cleavages among peasant groups.

The chapter on the coup d'état and its aftermath makes tragic reading. Prominent left-wing leaders were shot and many more were imprisoned and tortured. Many expropriated farms have been returned to their former owners. Unemployment, destitution, and malnutrition are once again a common feature among the rural poor. The Chilean coup was indeed a tragedy and it is to be hoped that history will, sooner than later, judge those who committed this crime against humanity.

CRISTÓBAL KAY
University of Glasgow

Festschriften and Miscellanies

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

BILL BRUGGER, editor. *China: The Impact of the Cultural Revolution*. New York: Barnes and Noble. 1978. Pp. 300. \$19.50.

GRAHAM YOUNG, Party Building and the Search for Unity. DENNIS WOODWARD, Political Power and Gun Barrels—The Role of the PLA. SYLVIA CHAN, Revolution in Higher Education. JOSEPH (YU-SHEK) CHENG, Strategy for Economic Development. DENNIS WOODWARD, 'Two Line Struggle' in Agriculture. ANDREW WATSON, Industrial Management—Experiments in Mass Participation. GREG O'LEARY, Chinese Foreign Policy—From 'Anti-Imperialism' to 'Anti-Hegemonism.'

A. C. DUKE and C. A. TAMSE, editors. *Britain and the Netherlands. Volume 6, War and Society. Papers Delivered to the Sixth Anglo-Dutch Historical Conference*. The Hague: Martinus Nijhoff. 1977. Pp. viii, 256. £38.45.

C. S. L. DAVIES, The English People and War in the Early Sixteenth Century. A. TH. VAN DEURSEN, Holland's Experience of War during the Revolt of the Netherlands. J. S. MORRILL, The Army Revolt of 1647. J. AALBERS, Holland's Financial Problems (1713–1733) and the Wars against Louis XIV. S. SCHAMA, Municipal Government and the Burden of the Poor in South Holland during the Napoleonic Wars. M. G. BUIST, The Sinews of War: The Role of Dutch Finance in European Politics (c. 1750–1815). G. F. A. BEST, Britain and Blockade, 1780–1940. J. S. BROMLEY, Away from Impressment: The Idea of a Royal Naval Reserve, 1696–1859. F. C. SPITS, Problems of Defence in a Non-Belligerent Society: Military Service in the Netherlands during the Second Half of the Nineteenth Century. A. MARWICK, World War II and Social Class in Great Britain. J. C. H. BLOM, The Second World War and Dutch Society: Continuity and Change.

KARL HAMMER and JÜRGEN VOSS, editors. *Historische Forschung im 18. Jahrhundert: Organisation, Zielsetzung, Ergebnisse*. 12. Deutsch-Französisches Historiker-

kolloquium des Deutschen Historischen Instituts Paris. (Pariser Historische Studien, number 13.) Bonn: Ludwig Röhrscheid Verlag. 1976. Pp. 484.

FRITZ WAGNER, Der Wissenschaftsbegriff im Zeitalter der Aufklärung. BRUNO NEVEU, Mabillon et l'historiographie gallicane vers 1700: Érudition ecclésiastique et recherche historique au XVII^e siècle. GÜNTHER SCHEEL, Leibniz und die deutsche Geschichtswissenschaft um 1700. PIERRE GASNAULT, Les travaux d'érudition des Mauristes au XVIII^e siècle. LUDWIG HAMMERMAYER, Die Forschungszentren der deutschen Benediktiner und ihre Vorhaben. DIETER GEMBICKI, Das "dépôt des chartes" (1762–1790). Ein historisches Forschungszentrum. HENRI DURANTON, La recherche historique à l'Académie des Inscriptions: L'exemple de l'Histoire de France. ANDREAS KRAUS, Die Geschichtswissenschaft an den deutschen Akademien des 18. Jahrhunderts. DANIEL ROCHE, L'Histoire dans les activités des académies provinciales en France au XVII^e siècle. PETER STADLER, Die historische Forschung in der Schweiz im 18. Jahrhundert. MANFRED SCHLENKE, Anfänge einer wissenschaftlichen Geschichtsschreibung in Grossbritannien im 18. Jahrhundert. JÜRGEN VOSS, Das Elsass als Mittler zwischen deutschen und französischer Geschichtswissenschaft im 18. Jahrhundert. ROBERT MANDROU, La Méthode historique de Voltaire, une lecture de "siècle de Louis XIV." EMILIO BUSSI, Die wissenschaftliche Methode in der Lehrart des Staatsrechts und des Reichsrechts im Zeitalter der Aufklärung. OTTO DANN, Das historische Interesse in der deutschen Gesellschaft des 18. Jahrhunderts. Geschichte und historische Forschung in den zeitgenössischen Zeitschriften. RUDOLF VIERHAUS, Geschichtsschreibung als Literatur im 18. Jahrhundert. NOTKER HAMMERSTEIN, Der Anteil des 18. Jahrhunderts an der Ausbildung der historischen Schulen des 19. Jahrhunderts.

FRED HIRSCH and JOHN H. GOLDTHORPE, editors. *The Political Economy of Inflation*. Cambridge: Harvard University Press. 1978. Pp. xi, 307. \$14.00.

J. S. FLEMING, The Economic Explanation of Inflation. CHARLES S. MAIER, The Politics of Inflation in the Twentieth Century. RICHARD PORTES, Inflation under Central Planning. DAVID PIACHAUD, Inflation and Income Distribution. ALAN T. PEACOCK and MARTIN RICKETTS, The Growth of the Public Sector and Inflation. M. PANIĆ, The Origin of Increasing Inflationary Tendencies in Contemporary Society. SAMUEL BRITTON, Inflation and Democ-

racy. JOHN H. GOLDTHORPE, The Current Inflation: Towards a Sociological Account. COLIN CROUCH, Inflation and the Political Organization of Economic Interests. MALCOLM ANDERSON, Power and Inflation. FRED HIRSCH, The Ideological Underlay of Inflation.

E. W. IVES *et al.*, editors. *Wealth and Power in Tudor England: Essays Presented to S. T. Bindoff*. London: Athlone Press, for University of London. 1978. Pp. xxi, 248. \$24.75.

R. VIRGOE, The Recovery of the Howards in East Anglia, 1485 to 1529. E. W. IVES, 'Agaynst taking awaye of Women': the Inception and Operation of the Abduction Act of 1487. J. J. SCARISBRICK, Cardinal Wolsey and the Common Weal. G. R. ELTON, The Sessional Printing of Statutes, 1484 to 1587. HELEN MILLER, Henry VIII's Unwritten Will: Grants of Lands and Honours in 1547. R. J. KNECHT, Francis I, 'Defender of the Faith'? GINA ALEXANDER, Victim or Spendthrift? the Bishop of London and his Income in the Sixteenth Century. PHYLLIS HEMBRY, Episcopal Palaces, 1535 to 1660. M. J. POWER, The East and West in Early-Modern London. B. DIETZ, Antwerp and London: the Structure and Balance of Trade in the 1560s. J. J. GORING, Wealden Ironmasters in the Age of Elizabeth.

RUSSELL MCCORMMACH *et al.*, editors. *Historical Studies in the Physical Sciences*. Volume 9. Baltimore: Johns Hopkins University Press. 1978. Pp. 362. \$18.95.

MAURICE CROSLAND and CROSBIE SMITH, The Transmission of Physics from France to Britain: 1800-1840. KENNETH L. CANEVA, From Galvanism to Electrodynamics: The Transformation of German Physics and Its Social Context. GERALD HOLTON, Subelectrons, Presuppositions, and the Millikan-Ehrenhaft Dispute. ALAN J. ROCKE, Atoms and Equivalents: The Early Development of the Chemical Atomic Theory. ELIZABETH GARBER, Molecular Science in Late-Nineteenth-Century Britain. BRUCE R. WEATON, Phillip Lenard and the Photoelectric Effect, 1889-1911. DANIEL M. SIEGEL, Classical-Electromagnetic and Relativistic Approaches to the Problem of Non-integral Atomic Masses.

RUTH T. MCVEY, editor, assisted by ADRIENNE SUD-DARD. *Southeast Asian Transitions: Approaches through Social History*. (Yale Southeast Asia Studies, number 8.) New Haven: Yale University Press. 1978. Pp. ix, 242. \$17.50.

RUTH T. MCVEY, Introduction: Local Voices, Central Power. HEATHER SUTHERLAND, The Taming of the Trengganu Elite, EDILBERTO C. DE JESÚS, The Tobacco Monopoly in the Cagayan Valley, 1786-1881. ONGHOKHAM, The Inscrutable and the Paranoid: An Investigation into the Sources of the Brotodiningrat Affair. JOHN A. LARKIN, The Campampangan Zarzuela: Theater for a Provincial Elite. DOROTHY HESS GUYOT, Communal Conflict in the Burma Delta.

EDWIN S. RAMAGE, editor. *Atlantis: Fact or Fiction?* Bloomington: Indiana University Press. 1978. Pp. xiv, 210. \$10.95.

EDWIN S. RAMAGE, Perspectives Ancient and Modern. JOHN V. LUCE, The Sources and Literary Form of Plato's Atlantis Narrative. S. CASEY FREDERICKS, Plato's Atlantis: A Mythologist Looks at Myth. J. RUFUS FEARS, Atlantis and the Minoan Thalassocracy: A Study in Modern Mythopoeism. DOROTHY B. VITALIANO, Atlantis from the Geologic Point of View. HERBERT E. WRIGHT, JR., Glacial Fluctuations, Sea-level Changes, and Catastrophic Floods.

SOLOMON WANK, editor. *Doves and Diplomats: Foreign Offices and Peace Movements in Europe and America in the Twentieth Century*. (Contributions in Political Science, number 4.) Westport, Conn.: Greenwood Press. 1978. Pp. xi, 303. \$18.50.

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- WYNIA, GARY W. *The Politics of Latin American Development*. New York: Cambridge University Press. 1978. Pp. xi, 335. Cloth \$21.95, paper \$8.50.

Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than five hundred words. Communications concerning articles or review articles may be no more than one thousand words, and the editors reserve the right to impose a lower limit. All communications should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor." Comments not intended for publication should be included in the covering letter. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

TO THE EDITOR:

I gather from recent issues that you have discontinued the "Communications."

All of these shining, self-serving, gross, cunning, snobbish, verbose, impertinent, offensive, poisonous, egotistical, mendacious, vain, paranoid, crass, dishonest, bootlicking, backstabbing letters from fools and knaves! All of these conniving careerists of every stripe—pompous asses, pretentious phonies, troglodyte reactionaries, fake radicals, repellent women's libbers, arrogant mandarins, mindless *routiniers*, condescending *Allmeister*, racist superpatriots, turf-guarding monopolists, overspecialized lintpickers, academic hacks, professorial racketeers, calculating grantsmen, perennial publicity hounds, the young on the make, the old in a rut! Was it a clever public relations move to drop this revealing feature?

I will miss that colorful and comic relief in your grim and grey pages.

JOHN MAASS
Philadelphia

THE EDITOR REPLIES:

Our communications also take vacations. Although we have searched our records thoroughly, the editors have been unable to identify the personalities listed by John Maass. All contributors to the *AHR* are scholars exclusively dedicated to the unbiased and dispassionate quest for truth.

OTTO PFLANZE

TO THE EDITOR:

I respectfully dissent from the editorial policy of the *Review* that leaves uncapitalized the first letter of the word "Black." Whether as an adjective or as a noun, the word is nearly always used nowadays to refer to an ethnic group in precisely the same manner as the words "Chicano," "Jew," and "Jewish." Since the word "white" is almost never used in this manner, its lack of capitalization is irrelevant to the issue.

TIMOTHY L. SMITH
The Johns Hopkins University

THE EDITOR REPLIES:

Timothy Smith's caveat concerns the style used in copyediting his article, "Religion and Ethnicity in America" (*AHR*, 83 [1978]: 1155-85). The use of the lower case for both "black" and "white" is standard not only for the *Review*, but also for every scholarly journal whose recent issues we consulted: the *Journal of American History*, the *American Political Science Review*, the *Journal of Negro History*, the *Journal of Southern History*, the *Historian*, and the *Annals of the American Academy of Political and Social Science*. It is also the recommended editorial practice of the University of Chicago Press's *A Manual of Style* (12th ed., 1966), page 158.

OTTO PFLANZE

TO THE EDITOR:

Previous work by Friedrich Katz has been marked by careful research, reasoned argument, and cautious conclusions congruent with the evidence.

Therefore, it is surprising to find that "Pancho Villa and the Attack on Columbus, New Mexico" (*AHR*, 83 [1978]: 101-30) is so riddled with errors of fact and interpretation as to make Katz's startling, revisionist thesis—that Villa's main motive was to preserve Mexican independence—totally untenable. Four mistakes on the first page alone illustrate the basic flaws in Katz's methodology and argument. One is a simple matter of factual detail: the battle lasted about three hours, not six. (Haldeen Braddy, "Pancho Villa at Columbus: The Raid of 1916," *Southwestern Studies*, 3 [1965]: 16-30.)

A second mistake has more serious consequences. Katz reports that John J. Pershing invaded Chihuahua under presidential orders to capture Villa. Pershing's orders actually limited him to dispersing Villa's forces and said nothing whatsoever about capture. Woodrow Wilson, through his secretaries of state and war, made a public avowal to seize the guerrilla and a private demand to disperse his band. The diplomatic reasons for making such a distinction are beyond the scope of this discussion. Salient here is that in his first footnote Katz cites three works that contain this ignored information: Herbert Malloy Mason, *The Great Pursuit* (1970), 68-71; Frank Tompkins, *Chasing Villa* (1939), 70-72; and Josefina E. de Fabela, ed., *Documentos Históricos de la Revolución Mexicana*, 12 (1967): 243-44. Katz's solecism on Pershing's mission then leads him to the unwarranted conclusion that the Punitive Expedition had been a military failure.

Katz continues to err with the odd remark that Villa's raid was "the one instance of Latin American military intervention in the United States." By that statement one must assume he means that this was the first raid against the United States for allegedly political reasons, since Indians and Mexican bandits had been raiding for decades. But even that will not work. In the summer and fall of 1915, Mexicans invaded South Texas repeatedly, motivated by a revolutionary movement known as the "Plan de San Diego, Texas." (Charles Cumberland, "Border Raids in the Lower Rio Grande Valley, 1915," *Southwestern Historical Quarterly*, 57 [1954]: 285-311.) The raids became so frequent and the destruction so severe that by November 1915 the War Department had committed half of the mobile troops in the continental United States to frontier defense in South Texas. (Adjutant General to Chief Clerk, War Department, November 4, 1915, National Archives, Adjutant General's Office, File no. 2338954, Record Group 94.)

Although Katz makes no mention of the Plan de San Diego, he inadvertently gives us photographic evidence of its existence. A picture labeled "Villistas killed in the Columbus raid" appears on

page 116. Clearly visible on the photograph itself is the caption "Mexican Bandits Killed at Norias." The image actually shows three Anglos desecrating Mexican corpses following a raid by members of the Plan on August 8, 1915 against the southern headquarters of the enormous King Ranch in South Texas. The rider on the far left is Captain J. M. Fox, Texas Ranger Force. The picture sold as a postcard on both sides of the Rio Grande as a grim warning to the followers of the Plan to quit. (Testimony of R. B. Creager, Texas State Legislature, "Proceedings of the Joint Committee of the Senate and the House in the Investigation of the State Ranger Force, January 31, 1919," vol. 1: 364, Texas State Archives, Austin.)

The cumulative effect of distortions of fact and interpretation leads Katz into serious difficulties with his thesis. Villa's raid, coming in the wake of the Plan de San Diego, demands a consideration of Wilsonian military policy toward Mexico in terms of border security. From the outset Katz uses a dubious course of logic and selective dismissal of evidence that culminate in the conclusion that Villa attacked the United States to preserve Mexican independence. Such reasoning seems possible only by ignoring the context and concentrating on the exotic in Villa's raid.

The sources for Villa's motivation lie in the reminiscences of both participants and victims. Their collective testimony, corroborated by the military record, argues that Villa raided Columbus for supplies, revenge against Sam Ravel (a local merchant), and settlement of a score with the Columbus State Bank for claiming that his account lacked funds. The bank matter may have been a trick by German agents to induce the raid. (James A. Sandos, "German Involvement in Northern Mexico, 1915-1916: A New Look at the Columbus Raid," *Hispanic American Historical Review*, 50 [1970]: 70-89.)

From the information of one participant, José Orozco, Villa entered Columbus in the predawn hours of March 9, 1916, and, while part of his force attacked the military encampment, part entered the Commercial Hotel looking for Sam Ravel, and Villa with an escort including Orozco went to the bank. Orozco stood guard outside while Villa made his "withdrawal." (Sandos, "German Involvement in Northern Mexico, 1915-1916," 78-80.) After noting carefully Villista activity during the raid, Haldeen Braddy concluded that Villa's "New Mexico incursion is adequately explained as guerrilla foraging" ("Pancho Villa at Columbus," 34). Katz ignores these facts in a drive to ennoble Villa's reasons for raiding.

Despite his tortured logic and his exaggeration of obscure documents into matters of importance, the only way Katz can reconcile Villa's activities—

the stealing of horses and weapons from the army, the sacking of Sam Ravel's store, hotel, and other property, the burning of the town, the robbing of the bank, and the raping of Mrs. J. J. Moore after she had seen her husband killed and her baby murdered—with Villa's alleged nationalistic goal to protect Mexican sovereignty is to ignore previous research. (For the interview with a survivor who recounted the attack on the Moores, see Clarence Clendenen, *Blood on the Border* [1969], 209.)

Thirteen years ago, Braddy summarized the state of the debate on the question: "When the evidence so plainly points to a simple explanation, no purpose is served in either resurrecting outworn theories or creating new speculations to supplant them" ("Pancho Villa at Columbus," 37). The existing interpretation accords with the facts. Katz's revision does not.

JAMES A. SANDOS

PROFESSOR KATZ REPLIES:

James A. Sandos's letter challenges my interpretation of Pancho Villa's motives in attacking Columbus, New Mexico, the sources on which my interpretation is based, and the accuracy of some of the facts I cite in the course of the narrative. He is in error on all counts.

First, my article seeks to show that Villa's *primary* objective in launching an assault on U.S. territory was to sabotage an agreement he believed existed between the U.S. government and the Carranza regime in Mexico, an agreement that would have converted Mexico into a virtual U.S. protectorate. (Although no such pact existed, a plot along similar lines had, in fact, been developed by a senior U.S. State Department official, Mexican conservatives, and American businessmen. The plotters had approached Villa with their scheme, but he did not know that, when it was subsequently aired at a cabinet meeting in Washington, both President Woodrow Wilson and Secretary of State William Jennings Bryan rejected it.) My article also indicates that Villa's *secondary* objectives were to revenge himself on certain individuals that he felt had betrayed him and to obtain supplies for his forces. Sandos repeats well-known evidence that Villa indeed pursued these secondary objectives. He provides no evidence at all, however, to back up his assertion that these secondary objectives were primary and that the attack's primary objective, which I describe, did not exist.

Second, Sandos asserts that the sources I cite to demonstrate the existence of Villa's primary objective are "obscure." Since he did not commend me for rescuing them from this obscurity, he apparently means to suggest that they are obscure in meaning, authorship, location, or authenticity.

This is simply not the case. The records of the U.S. Department of State, the collections of major university archives, the diplomatic correspondence of the British, French, and German foreign ministries, private correspondence, and official reports cannot be described as "obscure" in any of these senses. They are just different from the sources that Sandos prefers. He prefers "the reminiscences of both participants and victims" of the Columbus raid, "corroborated by the military record." But, if I had limited my research to these records alone, I would have been able to do no more than confirm the existence of Villa's secondary objectives in the attack on Columbus and thus would have repeated the error of previous researchers, including Sandos, who have done just this.

(Even limiting the research to the sources Sandos prefers would have been enough to discover an important clue to Villa's primary objective in the attack. In the evidence gathered by the U.S. military to explain Villa's motives was a pouch taken from the body of a dead Villista officer. The pouch contained a letter from Villa to Zapata that clearly stated Villa's belief in a secret U.S.-Carranza pact to turn Mexico into a U.S. protectorate. The letter also states clearly that this pact was the *principal* reason for the attack he intended to launch against U.S. territory.)

Third, as to matters of fact, James Sandos makes three criticisms: (1) that Villa's attack was not the first military intervention by Latin Americans on U.S. soil; (2) that General John J. Pershing's expedition was a success, not a failure, as I claim, because he had not been ordered to capture Villa but rather merely to disperse his forces; and (3) that the attack on Columbus took three hours, not six. I will respond to each of these in turn:

1. Sandos cites as a prior Latin American military attack on U.S. territory the activities of adherents of the Plan de San Diego. This plan, according to its authors, was drafted in San Diego, Texas, and called for an uprising of Mexican-Americans against the U.S. government for the purpose of separating the southwestern part of the United States from the union and eventually joining that territory to Mexico. The two main leaders, Luis de la Rosa and Aniceto Pizaño, were U.S. citizens. And, although there is evidence to indicate that the Mexican factions were using this movement for their own ends and allowing its adherents to use Mexico as a base of operations, I still consider it as primarily Mexican-American. (See Charles H. Harris and Louis R. Sadler, "The Plan of San Diego and the Mexican-United States War Crisis of 1916: A Reexamination," *Hispanic American Historical Review*, 58 [1978].) Certainly, the Plan's activities cannot be compared with Villa's attack, in

which the regular forces of a movement contending for power in Mexico intervened in order, as Villa stated repeatedly, to defend Mexico's independence from the United States. (The photographs that accompany my article were selected by the editors of the *American Historical Review*; see the editor's reply, below.)

2. General Pershing was issued instructions both to capture Villa and to disperse his forces. On March 10, U.S. Secretary of War Baker ordered General Funston to send an expedition to Mexico with the purpose of dispersing Villa's forces. Nothing was mentioned in this order about capturing Pancho Villa himself. On March 11, though, the adjutant general wrote to Pershing as follows: "The Secretary of War has designated you to command an expedition into Mexico to capture Villa and his bandits." (John J. Pershing, Report of the Punitive Expedition to Mexico, June 30, 1916 to October 7, 1916, National Archives, Adjutant General's Office, Record Group 94, as quoted in Frank E. Vandiver, *Black Jack: The Life and Times of John J. Pershing*, 2 [1977]: 605.) I agree with Sandos, however, that the main criterion for judging the success of the Pershing mission should not be the capture of Villa but the dispersal of his forces. From this point of view, the Pershing expedition was not only a failure; it was counterproductive.

When Villa attacked Columbus, he had been abandoned by most of his army and his remaining forces had been reduced to only a few hundred men. On September 19, several months after the Punitive Expedition had penetrated Mexico, the State Department received a report: "Villa has not less than 4,000 men in the vicinity of Chihuahua" (U.S. State Department Files 81200/19233). On September 23, 1916, the State Department received another report: "Men from the whole mountain country are flocking to him [Villa] and he is getting stronger every day while the majority of the people in Chihuahua are pro-Villista" (U.S. State Department Files 81200/19249). During September Villa's troops took and held, for a brief time, Chihuahua City and in December captured and briefly held the even larger city of Torreon. On January 7, 1917, a few weeks before the Punitive Expedition left Mexico, supposedly having completed its mission of disbanding Villa's troops, the State Department received yet another report: "Villa has entirely recovered and is reported to have about 15,000 well-armed men, ample ammunition, machine guns, and artillery" (U.S. State Department Files 81200/20264). If this is success, I wonder what constitutes failure. The Punitive Expedition had achieved exactly the contrary of

what it set out to do. It converted Villa into a symbol of national resistance against foreign attackers.

Events in the city of Parral constitute a good case in point. Parral was the base of operations of Villa's fiercest opponents in the state of Chihuahua, the brothers Luis and Maclovio Herrera. After the split between Villa and Carranza in 1909, they sided with Carranza; and most of the revolutionary contingents from Parral joined them. When the Punitive Expedition entered the city on April 12, 1916, however, it was greeted by the strongest expression of pro-Villa feeling it had yet encountered. Thousands of inhabitants lined the streets and staged a demonstration, shouting "Viva Villa." The most serious blow the Pershing expedition ever dealt Villa was to give up and leave Mexico. At that point Villa's influence began to dwindle.

3. Most accounts of the Columbus raid indicate that Villa's forces crossed the U.S. border at about 1 a.m., arrived at Columbus and launched their assault at about 4 a.m., retreated from the town shortly after 6 a.m., and fought a continuous rear-guard action against U.S. forces that pursued them into Mexico until sometime between 9 a.m. and 10 a.m. Thus, the period of fighting lasted between five and six hours.

Ever since I began my research on Pancho Villa, one of my strongest impressions has been of the intense emotionalism that this revolutionary continues to arouse in Mexico. For some, scholars and nonscholars alike, he is still the Centaur of the North, the man who could do no wrong and whose defeat was due solely to the betrayal of disloyal associates and the perfidy of Woodrow Wilson. To others, he is a monster or, as one recently published book puts it, "The Fifth Rider of the Apocalypse." James Sandos's letter indicates that this emotionalism is not limited to Mexico. I have the feeling that, in the final analysis, what disturbs him most about my article is my intimation that Villa, however one may judge his actions, acted from patriotic motives when he attacked Columbus.

FRIEDRICH KATZ
University of Chicago

THE EDITOR REPLIES:

James A. Sandos is correct that the picture in question was not of Villistas slain in the attack on Columbus. The *Review* trusted the source from which the picture was drawn (Jessie Peterson and Thelma Cox Knoles, eds., *Pancho Villa: Intimate Recollections by People Who Knew Him* [New York: Hastings House Publishers, 1977]), which proved to be inaccurate. The *Review* regrets the error.

OTTO PFLANZE

TO THE EDITOR:

I was delighted to see the publication of the article, "The Evolution of Medieval Mentalities: A Cognitive-Structural Approach," by my esteemed colleague, Charles M. Radding (*AHR*, 83 [1978]: 577-97). I am constrained, however, to take issue with his thesis since it seems to me that his discussion of psychology is insufficient and its application to medieval mentalities is misleading. I would like, if I may, to give my rejoinder a title: "Piaget in Context: or, The Uses of 'Childish' Cognition."

Jean Piaget's studies led him to discover different structures of cognition corresponding to different age groups. Young children have no notion of intent, no idea of interiority, and no sense of the man-made nature of laws. These are simply observations. They do not necessarily lead to the conclusion that young children are incapable of the cognitive modes of older children. On the contrary, an equally valid view—suggested by Piaget and Bärbel Inhelder themselves—is that affectivity fuels the cognitive structure (*The Psychology of the Child*, translated by H. Weaver [1969], esp. 158). In other words, six-year-old Mary does not utilize "older" cognitive structures because these do not correspond to her six-year-old interests. Since biological maturation is of great importance in determining the things children are interested in at different ages and since one child's biology is much like another's, a "developmental sequence" may be discovered. But nothing in this series indicates incapacity: all that it shows is areas of interest and disinterest.

Thus, cognitive structures correspond to the issues of importance to children (and, by extension, adults). They are directly dependent upon the context. When six-year-old Mary thinks that Billy is worse than Harry because Billy inadvertently broke one hundred dishes while Harry intentionally broke one dish, this does not mean that Mary cannot think in terms of intention. It means (to give one possible interpretation, although one would have to know Mary to know if it were correct) that Mary is concerned with issues of bigness and smallness, which might be explained by her context. Mary is, let us say, passing through the Oedipal stage, where Mommy's and Daddy's size is crucial to defining the marital relationship. "When I *grow up*"—that is the crucial point—"then I will marry Daddy."

The same sort of contextual understanding of cognitive modes must be brought to the Middle Ages. Radding cites the three features of Piaget's modality of "moral realism" as characteristic of the early Middle Ages: the indifference to intention, the lack of a sense of interiority, and the belief in the infallibility of external authority. I

would like to give just two examples of interpretations that counter his own:

First, George T. Beech, in his *Rural Society in Medieval France: The Gâtaine of Poitou in the Eleventh and Twelfth Centuries* ([1964], 92-93), has discussed a court case in the Gâtaine in the eleventh century. The prior of la Peyratte and the nobleman Thibaud Garin had a dispute; and the castellan's court declared that the verdict was to be decided by battle. Both men chose pledges to fight for them; Thibaud selected a knight named Odo. Then Thibaud announced that he would not accept the court's judgment. The court declared Thibaud's claim forfeit, and the prior went "into the house of [Thibaud's] pledge, i.e., Odo" and took "away his shield and lance," later giving these weapons to his own pledge. In short, Odo paid with his arms for Thibaud's decision, even though Odo was an "innocent party" in the whole incident. Clearly, Odo's intention was of no importance to the prior of la Peyratte: Odo had not fulfilled his pledge, so Odo was stripped of his weapons.

How might Radding interpret this curious event? If I understand his article correctly, I expect that he would say that the prior of la Peyratte has given us another example of Lawrence Kohlberg's stage two moral reasoning, based on the mode of "moral realism" that Piaget had described for young children, and that its explanation lies in the fact that the primitive culture of the early Middle Ages was conducive to a low level of cognitive development (see pp. 583-84).

How did Beech, writing fourteen years ago, interpret this same incident? He wrote, "To be treated in such a way in the presence of the greatest men of the region must have been a humiliating experience and have reminded Odo, in a most convincing way, of his subservience to his superiors" (*Rural Society in Medieval France*, 93). In short, Beech noted that, in the context of the Gâtaine, hierarchy was far more important than intention. His position makes sense, given the recent social and political changes in the region: from being a society of "free men" subject to the "public" powers of the count, the Gâtaine in the eleventh century had become a "feudal" society of dependents, with a nobility subject to private castellans, a knighthood subject to the nobility, and a peasantry subject to all of the great men.

Second, Radding cites the legalistic religious life of the early Middle Ages in contrast to the interior spiritual ideal of the twelfth century. One of his examples (p. 585 n. 24) touches on some documents with which I have been working that concern Cluniac literalism. Saint Odo, a tenth-century abbot of Cluny, took so seriously the injunction in the Benedictine Rule that monks

should look down at the ground that he never looked up. How different this is from Saint Bernard's notion of interior humility! Odo, by the standards that Radding applies, is thus relegated to the early cognitive mode of moral realism.

But, in the context in which Odo was living (and obeying), Odo's behavior had its reasons. Odo was aware, above all, of the lawlessness of the men of his day. He was acutely sensitive to the violence of unrestrained strong men (who robbed and killed) and of unrestrained monks (who quarrelled and sinned). (See Odo, *Collationes*, in *Patrologiae Latinae*, 133: cols. 517-638, and John of Salerno, *Vita Sancti Odonis*, in *Patrologiae Latinae*, 133: cols. 43-86, esp. 57-58.) The breakdown of the Carolingian order had set both monasteries and princes free of any central authority. Odo meant to set an example of lawfulness that would not depend on any royal power for its enforcement. Instead, it depended solely on obedience to a holy text, in this case the Benedictine Rule. Thus, Odo was not particularly interested in interiority because the overriding issue for him was overt behavior. He did not have the luxury that Bernard had had of a relatively peaceful, orderly society. This helps explain, as well, Odo's emphasis on ready-made and external rules. On every hand Odo saw men who violated laws with abandon. Why should he be interested in advocating an internal, questioning attitude toward the few laws that did exist?

One might counter Radding on still another ground: one might argue that his examples are skewed, that there was a demonstrable interest in intention, interiority, and the inner logic of rules in the early Middle Ages. Such interest was not "almost invariably" (Radding's terminology, p. 589) based on defunct texts from late Antiquity. Saint Odo himself, for example, was very concerned about certain kinds of intentions. In his *Vita Geraldi*, the life of Saint Gerald of Aurillac, he presented the picture of a fighter who went to war only for the "right" reasons and with only the "right" intentions. Odo spelled both of these out in detail. He pointed out that fighting was normally "incompatible with religion," but in Gerald's case the matter was different because Gerald was acting in accordance with rules proper to him, because his heart was filled with interior piety, and because his intentions were good. (See Odo, *Vita Sancti Geraldi*, in *Patrologiae Latinae*, 133: cols. 659-704, esp. 646-47.)

But Radding would argue that exceptions do not disprove the rule; and surely only a statistically exact, random sampling can decide whether Radding's examples or my examples are more characteristic of the period. And, in the end, we would both be disproved, because neither of us knows what the majority of people in the Middle Ages

thought: they have left no written sources for us to analyze.

BARBARA H. ROSENWEIN
Loyola University of Chicago

TO THE EDITOR:

Any 300-page cultural history of the human body over the past two centuries is bound to omit more than it contains. Nonetheless, Vern L. Bullough faults Stephen Kern's *Anatomy and Destiny* (*AHR*, 81 [1976]: 814) for neglecting the "public health and sanitation background" to olfactory impressions as well as "much of the history of bathing." Bullough even adds "the social implications of tight lacing," though Kern does treat these. In the same vein, Bullough also cites four slip-ups by Kern. Three of them are Bullough's own. Kern does not imply by merely reporting a belief of Eduard von Hartmann's—that a first husband would influence a woman's later children—that it "was unique to the nineteenth century." Nor, by the same token, does Kern endorse the view of some other historians that "the sexual morality of the early nineteenth century was essentially that of the bourgeoisie." As regards his alleged failure to "understand the implications for Victorian sexual morality of the discovery of the third stage of syphilis," this discovery was post-Victorian. The *suspicion* was Victorian, though, and Kern devotes several pages to its moral implications. That leaves one slip-up for Kern. He (and Freud) did say *coitus interruptus* where *coitus reservatus* was indicated. For shame!

Besides nitpicking, Bullough narrows Kern's focus from the body to sex. Locked into his own sexual antiquarianism, Bullough ignores or misses Kern's historical thesis: that World War I was a solvent of old strictures surrounding the body. And from Kern's whole run of cultural figures Bullough singles out just Freud. Clearly, Kern's book was too much for Bullough.

RUDOLF BINION
Brandeis University

PROFESSOR BULLOUGH REPLIES:

If Rudolf Binion's *ad hominem* attack is to encourage readers for Stephen Kern's book, it will have served an important purpose since as I tried to indicate the book is well worth reading and includes information not readily available elsewhere. I stand by the review as I wrote it and leave to the reader to find out whether I misrepresented the statement about von Hartmann or whether Kern's book essentially deals with the bourgeois or whether there is really basic understanding of the public health and sanitation developments in the century.

On the other two items, however, I have to come

to my own defense. I did not narrow the focus of the book from body to sex; the narrowing is essentially proposed by Kern. The dust jacket of the book indicated that Kern through his study of Freud had come to realize that Freud's work was only a part of a much broader cultural assault on the repressive sexual morality that dominated most of the nineteenth century. In reading the book I did not see any reason to challenge the dust jacket's description. On the second issue, third stage syphilis—an item that seems crucial to understanding some of the fear of sexuality so prevalent in the last part of the nineteenth century—was described by Philip Ricord, whose work was published in 1838 and whose theory was essentially accepted by the later Victorians. The implication of the existence of third stage syphilis in all its horrors I hold (and so do most others who have examined the topic) was decisive in much of the late nineteenth-century agitation against prostitution as well as a major contributing cause to the hostility expressed toward sex.

I hope that Binion's attack on me served to ease his ulcers and that we can now get back to the serious business of scholarship.

VERN L. BULLOUGH
California State University,
Northridge

TO THE EDITOR:

Peter Kenez's review of my book, *The Unknown Civil War in Soviet Russia: A Study of the Green Movement in the Tambov Region, 1920–21* (*AHR*, 82 [1977]: 703), is so warped that the reader is denied an understanding of the subject itself or of its setting in history. The ill will springs partly from political feeling and partly from the reviewer's reluctance to recognize, after completing a study of the conflict between Reds and Whites (1919–20), that his civil war was not the end of the story but was followed by a second one of very different character. The general line of the review is that the subject is of no consequence and the book should not be read. The line is well chosen, for if the book is read anyway, it will demolish every one of the contentions made—with a single exception.

The reviewer makes bold to deny that there was an unknown or second civil war and thus a third force in the Russian Revolution; he merely acknowledges a number of peasant "disturbances" before the Communists, or rather Lenin, consolidated their position by capitulating to the peasant interests embodied in the Green movement in order to kill these "disturbances" through the New Economic Policy (NEP). Furthermore, Lenin characterized the "disturbances" (the term is not his) as a much graver threat to his government

than all of the White armies of Russia, to which in the course of the first civil war he had never capitulated. And his commander in the field advised him that operations in Tambov had to be conducted as in "war"—nothing less would do. A reader may well prefer the testimony of Lenin and Tukhachevskii to the opinion of the reviewer.

Nowhere have I contended that Antonov's twenty-one thousand lightly armed soldiers constituted in themselves a mortal threat to the Soviet regime. The threat came from the pull their presence in the field exerted upon elements in the Red Army, overwhelmingly peasant in character, and this consideration, of course, in conjunction with the other Green forces, impelled Lenin to execute his "retreat in disorder" from militant Communism into the compromise of the NEP.

The subordination of intellectuals in the Green movement is one of its distinctive features. Yet without domination by intellectuals there is no hope for a movement, according to Kenez. The peasants failed because intellectuals did not organize them or provide "positive goals." The entire experience of 1917 has been lost on the reviewer. In that year a vast number of intellectuals assumed sway over rural Russia; an ocean of vapid and discordant ideology was produced and a far-flung organization was effected. And what happened to the peasants? They were lashed to the chariot of war—the "positive goal" provided for them—and they refused to pull. Those in the army turned to the Bolsheviks, true wolves in sheep's clothing.

Similarly with respect to Communism, of which, according to Kenez, I have no understanding. "The Bolsheviks," he says, "in order to feed the workers, had no choice but to take food from the peasants." They had a choice of whether to take all they could lay their hands on or to leave enough to sustain incentive for work. The NEP proves that even the Soviet state fared better when the producer retained something from the fruits of his labor. Is it the author's lack of understanding of Communism or the reviewer's lack of knowledge of Russian history that is here in the foreground?

I have allowed Lenin to speak for himself in relation to the rural toilers. No other record could be so impressive. His attitude is set forth in a series of memorable citations, not one of which is mentioned by the reviewer, who shies away from them like a cat circling a saucer of hot milk. And, of course, no word about Orders No. 130 and 171, the outstanding revelations of the book. Let the reader examine the text of these decrees (pp. 267–69, 322–25), and he will understand the silence.

The reviewer condemns the space allotted in the book to "trivia" and cites two examples. Both are badly chosen. Instead of "pages," the location of the village in question takes twenty lines. And,

instead of determining whether Antonov had twenty thousand or thirty thousand men, I had also to weigh in the scales whether he had several thousand, five or six thousand, ten thousand, or forty or fifty thousand. What is excessive about four pages devoted to one of the primary indices of Antonov's showing against Communist tyranny? What is trivial about thus being able to establish a disparity of 2.5 to 1 in favor of the Soviets in place of ostensible equality of forces, which superficial research or Soviet partisanship would swallow without blinking?

The reviewer expresses irritation over what he views as belaboring the obvious. Thus it is unnecessary, according to him, to absolve SR politicians of responsibility for the uprising. Here everything is messed up. They have not been absolved. There were any number of SR and Left SR politicians who were up to their ears in the Tambov affair, only they were local officials, not those on the all-Russian scene whom the Soviet government especially wished to convict. Obviously, the matter is not obvious, least of all to the reviewer, who has missed the whole connection between local party men and the Union of Toiling Peasantry (STK). The book is full of information about this one peasant organization, yet the reviewer says peasant organization is a neglected subject. To have overlooked or rather closed one's eyes to something of this magnitude, even with the habit of flipping pages, is in itself sufficient evidence of the quality of this review.

Along with other baseless criticism, the review includes one true article in its bill of indictment: Antonov is left shadowy. The book brings this figure back from oblivion but, unfortunately, not out of the shadows that still surround his memory. The work of effacement and concealment has been too thorough. To gain such insight, the reviewer has been forced to read with some care part of one chapter in a book that contains thirteen.

OLIVER H. RADKEY

University of Texas at Austin

PROFESSOR KENEZ REPLIES:

Oliver H. Radkey believes that only a person who has not read his book carefully could fail to be impressed by his arguments. I admire his self-confidence more than his book.

PETER KENEZ

*University of California,
Santa Cruz*

TO THE EDITOR:

In his review of my book, *Politics or Principle: Congressional Voting on the Civil War Amendments and the Negro Measures, 1838-1869* (*AHR*, 82 [1977]: 1077-78), Leonard P. Curry has provided a careful anal-

ysis and has set forth his main reservations concerning its conception and execution. This review was done in a professional manner, and I am grateful for his care and thoroughness.

There are, however, several points of clarification that can be usefully made. First, the purpose of the study was "to remedy some of the gaps in existing scholarship by identifying by name those congressmen who voted for and those who voted against the amendments and by collecting evidence concerning the degree of previous voting consistency of congressmen who voted for and against the amendments" (p. 8). It was written in response to an article on Negro suffrage and Republican politics by LaWanda and John Cox in which they stated, "We do not know how many of these congressmen had earlier demonstrated, or failed to demonstrate, a concern for the well-being of free Negroes or a willingness publicly to support the unpopular cause of Negro suffrage" (*Journal of Southern History*, 33 [1967]: 329). They suggested that techniques used by Edward Gambill, Glenn Linden, and David Donald to identify Radicals and Moderates on the basis of their voting records in Congress could be adapted to clarify the position of Republicans with respect to the Negro. After reading this article I attempted to apply the techniques used in my study of Radicals to the voting records of congressmen on measures relating to the Negro from 1838 to 1864 and then to compare the results with their votes on the Civil War Amendments. Though these measures varied over the twenty-five years, each involved some aspect of treatment of the Negro—some called for abolition of slavery while others involved the Wilmot Proviso, the Fugitive Slave Law, and emancipation of slaves in the District of Columbia. They are all of the votes cast by congressmen on measures related to the Negro in these years. (I believe that my finding of consistent voting behavior among most Republican and Democrat congressmen merits the careful consideration of historians studying the parties in these years. I know of no other book that contains this information, and it is certainly the type of additional data the Coxes felt was needed.)

Second, there was one error in the classification of congressmen by party, three typographical errors, four misclassifications by state, and eighteen votes recorded incorrectly for two congressmen. Since I was dealing with 464 men and 9,273 votes, my rate of accuracy was over 99.5 percent.

Third, I have rechecked the four major raw data tables on which the study rests. The tables were accurate but there were several errors in the identifying of measures as pro-Negro and in the numbering of measures in the appendix. With the correction of these errors in indexing, which the

publisher has agreed to do in an addendum, the tables are 99.2 percent accurate, 72 errors out of 9,273 votes. This adjustment means that of the 67 Republican senators who voted on the amendments, the votes of 64 are correct. A shift of eight votes for the remaining three senators, six for Lane of Indiana, one for Sawyer of South Carolina, and one for Yates of Illinois changes their voting percentages by 5 percent, 3 percent, and 2 percent, respectively. It has a minimal effect on their overall voting record and does not shift them from their present voting positions.

In conclusion, I believe that this book provides useful information toward a re-examination of congressional motivation during the Civil War and Reconstruction years. Despite some errors, it is over 99 percent accurate in its identification of congressmen by state and party and their voting on pro-Negro measures and the Civil War Amendments. Curry and I do differ in our views concerning the value of this type of evidence and the final judgment must rest with the reader.

GLENN M. LINDEN
Southern Methodist University

PROFESSOR CURRY REPLIES:

Glenn M. Linden is justifiably concerned that his work should be perceived as inaccurate and inadequate when he has reason to believe that it is neither. The comments and explanations he so courteously presents deserve a full hearing and careful consideration, for no honest reviewer would knowingly do an injustice to any author and certainly the profession should not tolerate either sloppy or intemperate reviews. I am relieved that Linden generously acknowledges that my review of his *Politics or Principle* could not be so characterized.

I will deal first with his second point—the question of accuracy. I do not understand Linden to assert that I erred in stating, on the basis of the published data, that in a large number of cases the vote aggregations for specific congressmen did not conform to the individual votes recorded. He does, however—and, as we shall see, with good reason—defend the accuracy of both the recorded votes and the aggregations. To comprehend Linden's position it is necessary to understand that the data are presented in three segments: (1) the votes of congressmen on individual measures, recorded as yeas and nays; (2) "keys" for each measure stating whether the author considered a yea or a nay vote to be "pro-Negro"; and (3) an aggregation for each congressman showing the number of "pro-Negro" and "anti-Negro" votes. Unfortunately, a variety of typographical errors caused the "keying" of nineteen of the measures to be stated incorrectly in some fashion. A single such error sometimes affects as many as one hundred and fifty

individuals. Hence, since it is possible to move from Segment 1 to Segment 3 only by passing through Segment 2, anyone working from the published data would find significant discrepancies between the votes and the aggregations. But, since the author's analysis was based on the correct—rather than the published—"keys," he insists that the actual (as opposed to the apparent) error is insignificant. Linden has kindly supplied me with the corrections, which the publisher proposes to print as an addendum. I have altered the "key" items as indicated and rechecked the calculations; I find that, with these corrections, in only forty-eight cases out of a total of some nine hundred aggregations does the "pro-Negro"—"anti-Negro" division differ from the recorded votes. And, more significantly, in only twelve instances is the error greater than a single vote. Anyone who has dealt with such large amounts of data would surely conclude that such an error rate is minimal. There remain some other sorts of errors that need not be addressed here, but scholars wishing to use Linden's material can rest assured that, once they have the addendum in hand, his data have a high degree of accuracy.

Linden's first point is more difficult to deal with. He acknowledges that voting records do not reveal motivation—for example, politics or principle—but defends such analysis as indicating consistency. It is tempting to say merely that we must agree to disagree, but such a cavalier response does not come to grips with the serious question raised by Linden and its importance for future research. For it seems clear to me that voting performance can reveal patterns of consistency or inconsistency or inconsistency *only* if the votes are cast upon substantially similar issues. John and LaWanda Cox, in their 1967 article that inspired Linden to undertake this study, suggested that historians needed "to know how many of these congressmen had earlier demonstrated, or failed to demonstrate, a concern for the well-being of free Negroes or a willingness publicly to support the unpopular cause of Negro suffrage." But Linden's 1838–64 universes of seventy-one Senate and one hundred and twenty-five House votes are by no means as narrowly focused as the Coxes suggested. More than 80 percent of the Senate and more than 85 percent of the House measures relate to slavery, and I am firmly convinced that an antislavery vote is of slight significance in determining the voter's position at that time on such matters as black suffrage or civil rights for Negroes. When seeking to determine where individuals stood on a variety of issues at a given time, we can probably employ profitably almost any constellation of categories; but, when attempting to measure consistency of performance across relatively large time periods, it

appears to me to be essential to define the issues narrowly and, in so far as possible, to keep our data base constant. In such cases, smaller is often better.

I will give a single example. There were four votes taken in the Senate in the summer of 1862 dealing with the admission of Negro testimony—an issue closely related to the later Fourteenth Amendment. One of these provided for the admission of such testimony in the District of Columbia in proceedings related to the D.C. Emancipation Act; thus, the measure dealt with slavery and applied only to an area unrepresented in Congress. On that vote the Republicans split 25 to 2 in favor of Negro testimony. The other three measures, however, would have prohibited the exclusion of black testimony in some or all of the proceedings in the federal courts throughout the Union. On those votes just under one-half, exactly one-half, and just over one-half of the Republicans cast negative ballots. Moreover, ten of the fifteen Republicans who voted negatively on at least two of the three more general proposals were still in the Senate when the Fourteenth Amendment was before that body, and eight of the ten were very solid supporters of that measure. These facts, it seems to me, speak directly, though by no means fully, to the question of consistency over time.

I apologize to Glenn Linden and to the readers (if any still remain at this point) for the length of this response. But Linden is, as the Coxes suggested more than a decade ago, dealing with matters of considerable importance to our understanding of the formulation of Reconstruction legislation. His arduously collected data, the substantial accuracy of which is established by the correction of the typographical errors, will doubtless be of use to many other scholars. And certainly he is entitled to some measure of praise and sympathy for being the first to venture down this treacherous trail.

LEONARD P. CURRY
University of Louisville

TO THE EDITOR:

Irwin M. Wall's bizarre review of my book, *Les Français et la Guerre d'Espagne* (*AHR*, 82 [1977]: 1133-34) invites a few clarifications. The title, first of all, was decided not by the author (who had chosen another) but by J.-B. Duroselle, director of the Publications de la Sorbonne and now a member of the Institut de France. The bibliography (whose truncated form we all lament) was devised on the principle that a list of primary sources is more valuable to the scholar than a list of the secondary literature, and it is a pity that Wall can find nothing good to say about seventy-three pages of annexes and indexes. A cursory glance at the

bibliography, however, would show that I consulted the very latest volumes of the *Documents diplomatiques français* that had appeared at the time the book went to press, and the same is true for the updated Spanish edition published in Barcelona.

What is to be said of Wall's remark that the book is "often tedious . . . although a good deal is of interest"? Or the doubt he expressed that France in 1936-39 had any responsible newspapers, when the papers most quoted from are the illustrious *La Dépêche* and *L'Ordre*? Again, the very chapter headings explain to the reader the division within Blum's cabinet between the Cot formula and the Delbos-Daladier formula. Why the reference to Colton and the doubt that Blum was dissuaded by Baldwin, when Colton himself has written that "Blum followed the lead of Britain"?

Perhaps Wall does not realize that, when the *AHR* reviewed the English edition of 1968 (74 [1969]: 1249), a distinguished Hispanist, Stanley G. Payne, found much in it to commend. While I share Wall's disdain for the lies published in the pro-Fascist press, I suspect that he does not share my disdain when the other extreme distorts the truth. Suffice it to say that Payne's review called me "more friendly to the Left than to the Right," a viewpoint shared by *Le Monde Moderne* (Paris) and by Javier Rubio in their critiques of this work.

Finally, Wall attempts to drive a wedge between the author and his own *préfacier*, Pierre Renouvin. This eminent historian accepted very few of the manifold invitations he received, and certainly he never wrote any preface in order to "refute" the author's work. Scholars seeking a balanced appraisal of this book might consult the reviews in *Le Monde* or the *Times Literary Supplement*.

DAVID WINGATE PIKE
American College in Paris

PROFESSOR WALL REPLIES:

I do not feel it worthwhile to reply to David Wingate Pike, whose observations do not speak to the substantive points raised in my review. I would, however, like to allay his suspicion and to assure him that I dislike distortions of the truth from whatever quarter they may come.

IRWIN M. WALL
*University of California,
Riverside*

TO THE EDITOR:

The review of my book *The Zapotecs* by Douglas Butterworth (*AHR*, 82 [1977]: 1371-72) requires some response. It is unfortunate that Butterworth chose to waste much of the short space allotted to him on minor points of omission and error (although I am grateful to him for bringing my atten-

tion to one such error concerning *pulque*). In his hands, such an approach resulted in a number of misleading representations of both the content and character of my book.

For example, Butterworth implies that I did not utilize nor consult the works of Kent V. Flannery and John Chance. If this was, in fact, his implication, he is in error. The works of Flannery and his colleagues are cited in numerous places and Chance's work may be found in the first note of chapter 5. To be sure, I chose not to cite material that appeared after my book had gone to press.

Other remarks are also misleading. In discussing the various meanings of the term "Zapotec," Butterworth contends (after presenting what appears to represent his own thoughts on the matter) that "Whitcotton touches upon this problem." He then presents a quotation from the epilogue of the book, implying that I deal very little with the problem of meanings. In fact, I devote part of each chapter to this topic.

Butterworth's final cryptic remark concerning my contribution to original research on the subject matter puzzles me, especially since he knows that my primary research effort in the Zapotec field has been in post-Classic ethnohistory. He glosses over the two chapters in which this research is summarized with the remark that "the strongest parts of the book are chapters three and four."

Since materials on the Zapotecs seemed un-integrated and relatively abundant, my primary intent in the book, as stated in the preface, was to integrate and analyze the various materials gathered on this Oaxacan people by archeologists, ethnohistorians, linguists, ethnographers, and social anthropologists. And mine is the first book to take an integrative approach to this diverse literature. The sources I utilized in the preparation of *The Zapotecs* are clearly identified in some fifty pages of notes and bibliography, a feature also not mentioned by Butterworth in his excessively pedantic review.

JOSEPH WHITCOTTON
University of Oklahoma

TO THE EDITOR:

Charles D. Cashdollar's review of my book, *Octavious Brooks Frothingham, Gentle Radical* (AHR, 83 [1978]: 285-86), was, I believe, unduly caustic. It is true that there are inconsistencies and errors in the spelling of the proper names of some individuals. From this review and others, however, it seems that, if the reviewer is not in sympathy with the subject, the tendency is to dwell mainly on procedural errors. Not much can be said to answer this sort of criticism. But Cashdollar's substantive criticisms can be answered, and I can, I think, help modify the negative impression he created.

Cashdollar claims that I did not relate Frothingham's ideas clearly to other thinkers, especially to Theodore Parker and Herbert Spencer. First, I made it quite clear that Frothingham's was not an original but a derivative, eclectic mind. Since I was not writing a comparative study, there was no reason, nor was there any clear evidence, to cause me to trace his ideas back to the German and English philosophers of the Romantic era. In discussing his Harvard background, however, I did point out that his generation was brought up in the post-Lockean, antisensational school of philosophy—in the Romantic-intuitional challenge to the empiricism of the Enlightenment.

Second, as for Parker's influence, there is no clear evidence that Parker turned Frothingham into a religious radical. It is possible, as I mentioned in the book, that he and his classmates heard Parker as an "extra-curricular" activity; but neither in Frothingham's papers nor in Parker's letters and journals is there evidence to justify a clear connection of intellectual influence. I am sure that there was some, as others (mainly Henry Steele Commager) have assumed; but they have assumed too much. An honest biographer cannot make assumptions. In fact, in my book I quoted from Frothingham's Sunday school lectures to some Salem ladies in 1846-47 (his last year in seminary) that demonstrate that he was not orthodox and can be considered radical six years before meeting Parker in 1854. As for the direct influence of Herbert Spencer, it is very vague and not possible to document. Spencer's ideas dominated the thinking of Frothingham's generation. He was certainly aware of the evolutionary theory and the Spencerian-competitive ethic in society. He spoke of the "Development Theory" and the "New Social Sciences" in numerous sermons and articles. All of this was brought out in the biography.

Third, Stow Persons has called Frothingham's *Religion of Humanity* a "representative essay in free religion." True, but as such it is a comprehensive summary of Frothingham's theological views and his attacks on orthodoxy. For this reason I gave it "passing reference," as Cashdollar says. Rather, I devoted a chapter, "The Pulpit Voice," to presenting in some detail Frothingham's views on theological categories, social issues, and ethics. I digested, quoted, and paraphrased about three hundred of his sermons. If the ideas are "incomplete and misleading," then so was Octavious Brooks Frothingham. As a matter of fact, this reviewer (and others) have leveled the same kind of substantive criticism at the biography that was leveled at Frothingham himself during his active years of combatting religious orthodoxy and narrow sectarianism. Some people, then and now, cannot grasp the concept of an open mind where there are few discernible

outlines of a clear-cut body of ideas. Frothingham said his creed was creedless, which confused his critics and, apparently, those of his biographer.

J. WADE CARUTHERS
Southern Connecticut State College

TO THE EDITOR:

I would like to clarify a statement in J. Woodford Howard's review of Gerald T. Dunne's *Hugo Black and the Judicial Revolution* (*AHR*, 82 [1977]: 1352) about the destruction of Justice Black's papers. Justice Black's conference notes only were destroyed by the family in accordance with his wishes. Fortunately, a voluminous collection of papers remains, including many case files and other "judicial papers." In 1973 they were donated to the Library of Congress by the Black family. A de-

scription of the papers can be found in the *Library of Congress Quarterly Journal*, 30 (1973): 308-10.

JOHN C. BRODERICK
Library of Congress

ERRATUM:

The review of Hugh and Christopher Seton-Watson's *R. W. Seton-Watson and the Yugoslavs* (*AHR*, 82 [1977]: 1015) erroneously identified the Seton-Watsons as father and son, not as brothers. The headnote to that review inadvertently omitted the names of the four co-editors: Ljubo Boban, Mirjana Gross, Bogdan Krizman, and Dragovan Šepić. The *Review* apologizes for both the error and the oversight.

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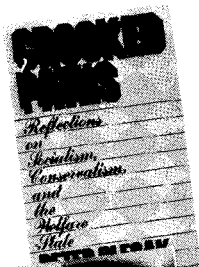


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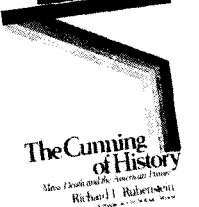


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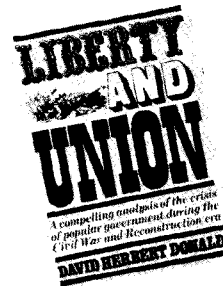
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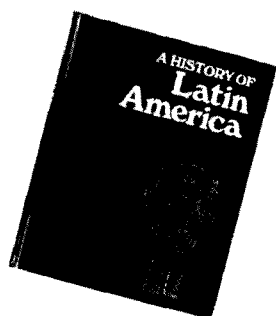
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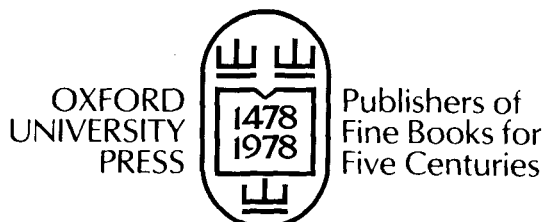
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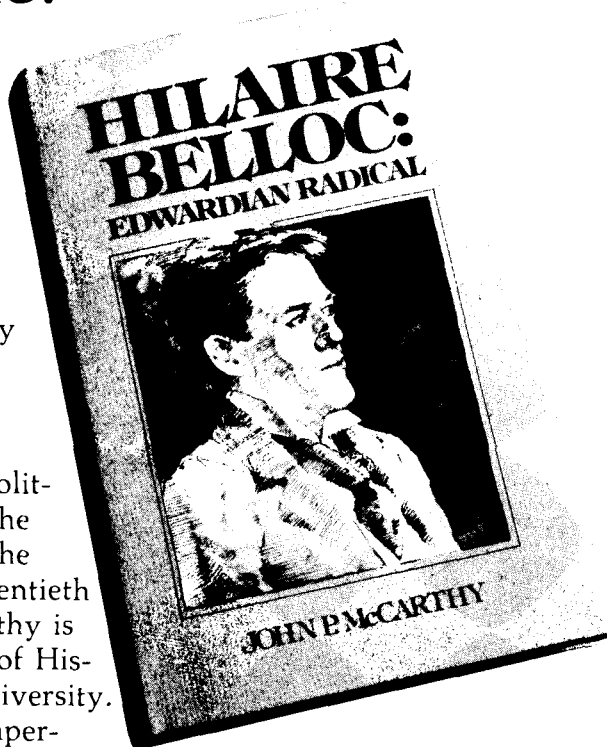
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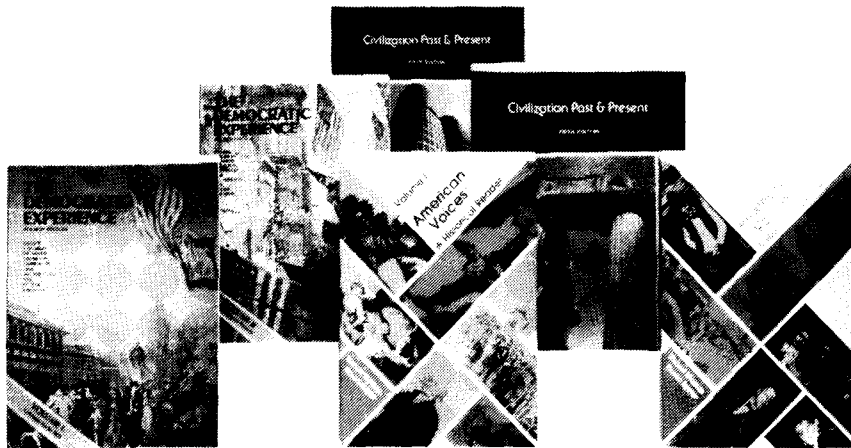


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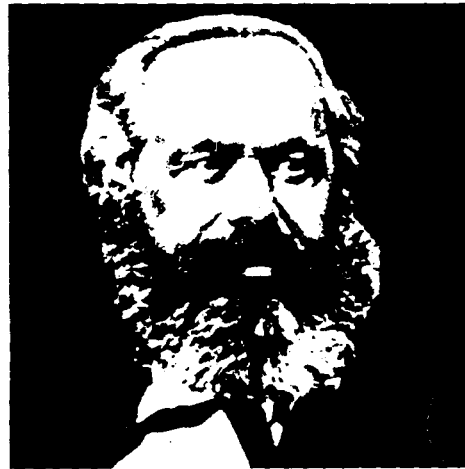
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